

PHILOSOPHICAL
TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

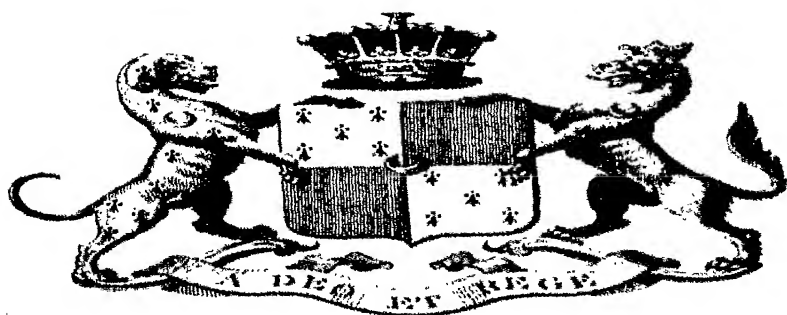
FOR THE YEAR MDCCCXVIII.

PART I.

LONDON,

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MDCCCXVIII.



ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done, upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them ; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities, of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices ; which in some instances have been too lightly credited, to the dishonour of the Society.

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ERRATA.

- Page 72, under " Corr. for arc," line 2nd, for 1,15 read 1,51.
 under " vibrations for 24 hours," line 2nd, for 86056,93 read 86057,29.
- Page 86, " Great weight above," for mean 86057,70, read 86057,79.
 Experiment B, for 86057,70, read 86057,79.
- Page 87, Experiment B, under " Diff." for ,23 read ,14.
 Experiment B, for 86059,93, read 86057,93.
- Page 116, line 22, for " constitute," read " constitutes."

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on SIR GODFREY COPLEY's Donation, for the year 1817, to CAPTAIN HENRY KATER, F. R. S. for his Experiments on the Pendulum.

And they adjudged the Gold and Silver Medals, on the Donation of BENJAMIN COUNT of RUMFORD, to SIR HUMPHRY DAVY, LL.D. F. R. S. for his Papers on Combustion and Flame, published in the last Volume of the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *On the great strength given to Ships of War by the application of Diagonal Braces.* By Robert Seppings, Esq. F. R. S.

Read November 27, 1817.

SINCE the time that I first suggested the principle of applying a diagonal frame-work to ships of war, which was first partially and successfully adopted in the Kent, a seventy-four gun ship, in the year 1805, my mind has been continually and anxiously turned to this important subject; and it was not until the utility of the experiment had been fully established in the opinion of most naval officers, that I ventured to present to the Royal Society, a paper on the application of this well known principle to the construction of large ships of war, but which, as far as my knowledge extends, never had before that time been so applied, either theoretically or practically, in this, or any other maritime country; and I am well assured, that no such application, or suggestion, appears in any of the Continental writers on naval architecture. I merely mention this, because it has been pretty broadly insinuated, that the idea was borrowed from the French. The propriety of a different disposition of

the materials entering into the construction of a ship, has at different times, for more than a century past, been suggested by English ship-builders; and partial alterations have, in consequence, been introduced; but no one, that I am aware of, has at any time proposed the system of a diagonal trussed frame. If I have received any assistance in the progress of this new system, now universally adopted in the British navy, it was from the plans and drawings of the celebrated bridge of Schaffhausen, and from no other source.

The extensive application of this principle to no less than thirty-eight sail of the line, and thirty frigates, might perhaps be conclusive as to the advantages expected to be derived from the new system; but as the Royal Society did me the honour to introduce my account of that system into their Transactions, at an early period of its adoption, I am led to hope that the result of a practical experiment, made with a view of proving the correctness of the principle, may not be deemed an improper or an uninteresting corollary to my former paper.

In the early part of this year (1817) the *Justitia*, an old Danish seventy-four gun ship, was ordered to be broken up on account of her defective state; and having observed her to be considerably arched, or hogged, as it is usually termed, I determined, notwithstanding her age and defective state, to apply the trussing principle to a certain extent, with a view to observe what effect it would produce on a fabric reduced to so weak and shaken a condition.

The officers of the yard were directed to place sights on the lower and upper gun-decks, prior to her being taken into the dock; and to ascertain, when she grounded on the

blocks, how much she had altered from the state in which she was when afloat. They were then to place a certain number of trusses (conformably with the annexed drawing, Plate I. No. 1.) in the following manner: those in the hold marked A, to be placed in an angle of 45° , or thereabouts, and those marked B, at right angles to them; those in the ports marked C to be placed from the midships forward, in an angle of about 40° , and, from the midships aft, at the same angle, but in an opposite direction. As it was uncertain where the centre of fracture (or point of separation) would take place, a few of the port-holes about the centre of the ship were crossed, as shown in Plate I. at D. Wedges were applied to the heels of the trusses, which were then set tight. The ship being thus partially trussed, the water was then to be let into the dock, and the ship floated out of it into the bason, where she was to lay one hour, when a committee was to examine the sights, and ascertain how much the ship had altered; and again, what change had taken place in twenty-four hours after floating. This being done, the trusses were to be disengaged in as short a time as possible, in order to observe whether the effect of their removal would be instantaneous, or gradual.

The following is an extract of the report of the committee:

“When the ship was in dock, on blocks perfectly straight,
“ she came down in the midships, by the sights placed on
“ the gun-deck, two feet two inches and a half; and by
“ those on the upper deck, two feet three inches and a quar-
“ ter; and when undocked, with the trusses complete, and
“ in their places, she hogged, or broke her sheer, by the
“ sights on the gun-deck, one foot two inches; and by those

extract of a letter from Captain Ross of the *Northumberland*, to Sir GEORGE COCKBURN, which was transmitted to the Admiralty :

“ I have to state, that the fore and aft side required caulking on the passage from England (which was partially done) when the diagonal side did not ; the fore and aft side now requires caulking all over, and the diagonal side very little ; being, in my opinion, and that of the carpenter, much in favour of the diagonal decks.”

On the return of the *Northumberland* to Sheerness, the officers of the yard were directed very particularly to survey her decks. After speaking of the favourable report made to their enquiries by the officers of the ship, they stated as follows :

“ This report of the officers was confirmed by the general appearance of the ship on her arrival at this port, and having subsequently caulked and minutely examined the state of the decks and water-ways, we find the comparison so much in favour of the larboard side, as to determine, that the diagonal system of laying decks is preferable to the common system.”

- II. *A memoir on the geography of the north-eastern part of Asia, and on the question whether Asia and America are contiguous, or are separated by the sea. By Captain James Burney, F. R. S.*

Read December 11, 1817.

A BELIEF has prevailed for nearly a century, that the separation of America and Asia has been demonstrated by an actual navigation performed; and it is distinctly so admitted in the charts. It is proposed to show in this memoir, in the first place, that there does not exist satisfactory proof of such a separation; and secondly, that from peculiarities which have been observed, there is cause to suppose the fact to be otherwise; that is to say, that Asia and America are contiguous, and parts of one and the same continent. This is not an opinion newly formed, but one which many years ago was impressed on other persons as well as on myself, by circumstances witnessed when in the sea to the north of Bering's Strait with Captain Cook, in his last voyage.

America, from its first discovery by the people of Europe, was regarded by them as a land wholly distinct from their own native continent, till the failure of many attempts to discover a northern passage to India at length suggested the possibility that the Old and New World (as they were then called) formed but one continent. The solution of this problem, so far as regards a north-eastern navigation to India, has been more naturally the business of the Russians

than of any other people, as well on account of the greater facilities possessed by them for prosecuting the discovery, as for the superior benefit they would derive from a practicable navigation round their coasts to the Tartarian and Indian sea, should such be found.

The memorable voyage of SEMOEN DESCHNEW and his companions in 1648, by which the Russians first discovered the sea east of Kamschatka, (for before that time the river Anadir was supposed to run into the Icy sea), is the principal circumstance which has been admitted as proof of a complete separation of Asia and America. It is important to remark, that this admission is not so old as the expedition on which it is founded, by nearly a century; for no certainty of an absolute navigation having been performed round a north-eastern promontory and extremity of Asia was pretended till after the year 1736, when it was inferred by Professor MULLER, from some original writings found at that time in Siberia, concerning DESCHNEW's voyage. Baron de STRAHLENBERG, who had lived many years in Siberia, and whose description of that country is of earlier date than MULLER's publication, says of the expedition of 1648, that some Russians departed from the river Lena in boats towards the east, and by that route discovered Kamtschatka. But it was not understood to have been by a clear navigation round the N. E. of Asia; for in a description subsequently written, he says, "a class of people, to whom has been given the denomination of Tartars, inhabit the north-eastern extremity of Asia, concerning which a Kossak officer, named Atlassow, reported, that between the Kolyma and the Anadir were two great promontories, which he affirmed could not both be

“doubled by any vessel, because the west coast of the first
“is barred in the summer by floating ice, and in winter the
“sea there is frozen; but at the second, the sea is clear,
“without ice.”

SCHEUCHZER, the translator of KÆMPFER's History of Japan, in an introduction to his translation, cites some remarks which had been published concerning the Tartars, wherein it was said, “the inhabitants of Siberia who live near
“the river Lena, and along the coast of the Icy ocean, in
“their commerce with Kamtschatka, commonly go with
“their ships round a Suetoi Noss [or sacred cape], to avoid
“the Tschelatzki and Tschuktzki, two fierce and barbarous
“nations possessed of the north-east point of Siberia.” On this vague authority SCHEUCHZER concludes, that Asia is not contiguous to America.

When Mr. MULLER first went into Siberia, no credited tradition appears to have been there current of the north-east extremity of Asia having been sailed round. Charts which were made in Siberia by people inhabiting the coasts of the Icy sea, showed *uncertainty*, and what is to be considered only as an expression of a *belief* of a great north-eastern promontory; for at that part, the coast was not defined by any outline, but left without limitation: whereas a more southern promontory, supposed the second from the Kolyma, was clearly delineated in the charts without any indication of doubt; and this last-mentioned promontory, it is evident, was the cape which was afterwards seen by BERING, and to which Captain Cook gave the name of Cape East, on account of its being the most eastern land known of Asia. In the instructions which were given by the Czar PETER the Great

for Captain BERING's voyage, the question whether Asia and America were contiguous or separate, was regarded as *undetermined*, and some Tschuktzki people, with whom BERING had communication, informed him that, "their countrymen
 " who traded with the Russians on the river Kolyma, always
 " went thither by land with their merchandize on sledges,
 " drawn by rein-deer, and that they had never made the
 " voyage by sea."

Mr. MULLER has acknowledged that from the perusal of the papers found concerning the voyage of DESCHNEW, he adopted a belief which did not before prevail, and he regarded it as a second discovery. Yet Mr. MULLER's own account fell very short of warranting a *certainly* of the manner in which DESCHNEW arrived at the Eastern Sea; and there is an irregularity in it which is perplexing. He says, 'DESCHNEW in relating his adventures speaks only incidentally of what happened to him by sea. We find no event mentioned till he had reached the great cape of the Tschuktzki. His relation,' says Mr. MULLER, 'begins at this cape. It lies between the north and north-east, and turns circular towards the river Anadir. Opposite to the cape are two islands, on which were seen men through whose lips were run pieces of the teeth of the sea horse. With a favourable wind one might sail from here to the Anadir in three days and three nights.'

The cape or promontory which is here described is evidently the Cape East in Bering's Strait; and in a subsequent part of the account, DESCHNEW is represented to have said that this Noss 'on which the vessel of ANKUDINOW, (one of his companions) was wrecked, was not the first promontory

that had occurred, to which they had given the name of *Swiætoi Noss*.^{*} The word *Swiætoi* signifies sacred, and is a name suitable to a promontory which could not be doubled. And this corresponds with the Siberian charts before noticed.*

It is necessary here to explain by what means the navigators in the Icy sea were enabled to arrive with their vessels at a second promontory, without having sailed round the first. On account of the frequency of being inclosed in the Icy sea, by the drift ice, it was customary to construct vessels in a manner that admitted of their being with ease taken to pieces; by which they could be carried across the ice to the outer edge, and there be put together again. The planks were fastened and kept to the timbers only by leathern straps, in lieu of nails or pegs. The construction of the vessels in which *DESCHNEW* and his companions went is not specified. *MR. MULLER* calls them *Kotsches*. *BARON STRAHLENBERG* says they departed eastward from the river *Lena* in their *boats*.

In the beginning of the 18th century, the Czar *PETER* the Great sent directions to the Governor of *Iakutzk* to collect information concerning the discoveries which had been made. In consequence of this order, several examinations and depositions were taken; and the few authentic particulars which are known of the voyage of *DESCHNEW* were thereby preserved. The most remarkable of the depositions which are cited by *MR. MULLER*, next to what relates to the expedition of

* It may be objected to this inference, that another cape in the Icy sea, although it has been sailed round, bears nevertheless the name of *Swiætoi Noss*; but it may naturally be imagined that the name was given before the difficulty had been surmounted.

DESCHNEW, is one which was made by a person named NIKIPHOR MALGIN, who stated that "a merchant named "TARAS STADUCHIN, did many years before relate to him, the "deponent, that he had sailed with ninety men in a Kotsche "from the river Kolyma towards the great cape of the "Tschuktzki: that not being able to double it, they had crossed "over on foot to the other side, where they built other vessels. "The small breadth of the isthmus at the part where they "crossed, is noticed as the most remarkable circumstance in "this deposition." They afterwards proceeded along the coast round the Kamtschatka Peninsula, till they came to the Penschinska gulf; and, in the short account which is given of this navigation, is found, expressed in an obscure manner, the *first* notice obtained by the Russians of the Kurilski islands.

This is a clearly described passage. Besides the expedition of DESCHNEW, and this of TARAS STADUCHIN, only one other instance is mentioned of any vessel having gone by sea from the Kolyma round the Tschuktzki coast; and this last mentioned case rests on the authority of an unauthenticated tradition, purporting that some man had gone in a vessel not larger than a skiff, from the Kolyma to Kamtschatka; and no other particular is spoken of in the report.

This was the state of the information obtained concerning the north-eastern extremity of Asia, at the time of Captain BERING's voyage. The Asiatic side only of Bering's Strait was discovered in that voyage, and the coast of Asia being there found to take a western direction, it had the effect of giving an impression, equal to demonstration, of a total separation of Asia and America. And after that time, and not

before, DESCHNEW was believed to have performed the whole of his voyage from the Kolyma to the Anadir by sea.

Many reports had circulated in Siberia of the existence of northern lands in the Icy sea; but persons sent purposely to examine, had not found land, which much discredited the reports. A chart in which a northern land was marked was however published at Petersburg, about the year 1626, by a Colonel SCHESTAKOW, of the Jakutzk Kossaks, a man of great ability as well as enterprise. Neither SCHESTAKOW nor his chart, however, are favourably noticed by Mr. MULLER, who was in general a candid historian. On SCHESTAKOW's chart, the north land was marked with the name of the Large Country. M. de LISLE gave credit to SCHESTAKOW's map for the Large Country, which he makes appear on his own chart as a part of America, extending westward beyond the Kolyma.

Between the years 1734 and 1739, three expeditions were undertaken to ascertain the limits of Asia to the north and north-east, from which no advantage was reaped, and they were attended with circumstances of extraordinary distress and misery. These undertakings show that the boundary of Asia was not then regarded as ascertained. In 1764, a chart was sent from Siberia to Petersburg, which again showed a continuation of the American continent stretching far to the west, and opposite to the Siberian coast of the Icy sea.

Between the years 1760 and 1765, no less than four attempts were made by one and the same individual, a Russian merchant, named SHALAUROF, to sail from the Icy sea round the north-east of Asia. In the last of these attempts this en-

terprising and persevering man perished, for neither himself nor any of his people ever returned.

The information which was obtained in the first three attempts of SHALAUROF, is simply, that he arrived at an island which he named Sabedei, and beyond it sailed into a bay of the continent, which he named Tschaoon bay, which was estimated to be distant about 70 leagues to the east from the entrance of the river Kolyma. Here were found habitations and people.

Tschaoon bay ran deep into the land southward and eastward, and probably it was from this place that TARAS SRADUCHIN crossed over to the eastern sea. Northward from Tschaoon bay, the coast took something of a westerly direction. The most advanced part of the land seen, was a high mountain far off to the north-east, SHALAUROF being then to the north of the island Sabedei.

Among the attempts to determine the north-eastern limits of Asia, is to be reckoned the march of a small Kossak army under the command of a Captain PAULUTZKI, which after traversing the Tschuktzki country, from the gulf of Anadir to the Icy sea, marched along the shore eastward, with intention to trace round the north-east coast; but the land being found to run far north, and their provision being expended, PAULUTZKI was obliged to relinquish the attempt.

Such was the state of the information which had been obtained, when Captain Cook arrived in the sea of Kamtschatka. Of three passages said to have been accomplished from the Icy sea to the Eastern sea, the manner of performing the voyage is distinctly expressed only in one; and that is speci-

fied to have been by crossing an isthmus, and not by sailing round a promontory.

I come now to speak of what was observed in the voyage of Captain Cook. The first extraordinary circumstance noticed on arriving in Bering's Strait, was a sudden disappearance of the tides. To the south of Bering's Strait, both on the Asiatic and on the American side, we had experienced strong tides. Near one of the Aleutian islands, where the ships had anchored, a tide was found running at the rate of seven miles per hour (as measured by the log) smooth and unruffled; at the same time, in the middle of the channel between this island and the next, the rapidity of the stream kept the waters in a foam during four hours of the tide.

Bering's Strait is formed at the narrowest part by two points, one named Cape Prince of Wales, which is the westernmost land known of America; and the other named Cape East, being the most eastern known land of Asia. Whilst we were to the south, and within sight of the Cape Prince of Wales, the wind and current, being in contrary directions, raised a sea that frequently broke over the ships. On arriving within Cape Prince of Wales, the ships anchored, the east cape of Asia then bearing due west; and it is remarked by Captain Cook, that whilst the ships lay there at anchor, which was from six to nine in the evening, there was found little or no current; nor could it be perceived that the water either rose or fell. Afterwards, whilst to the northward of Bering's Strait, we always had soundings of moderate depth, which enabled us to measure the stream with great exactness; and we seldom found one running at

the rate of more than half a mile per hour; at no time at the rate of a mile.

It is doubtless possible, that large bodies of ice taking the ground may choak up a channel between two seas, so as wholly to obstruct the tides; but it is not probable, that such should have been the case between this sea and the icy sea, through the whole month of August and the beginning of September, to which time Captain Cook remained in the sea north of Bering's Strait. And the same stillness of the waters was observed there in the ensuing summer. The bottom also, not being swept by streams, was of soft ooze, so tenacious that the sounding line in common use was not strong enough to disengage the lead, and it became necessary to sound with a smaller lead and stronger line.

From Bering's Strait, Captain Cook coasted the land of America, to the north and north-eastward, as near as weather and other circumstances would admit, till, in latitude 70° 40' N. his farther advance was stopped by a close body of ice to the N. and N. E. The ice, though compact, was not fixed, and was found to be approaching the American coast. Captain Cook remarks, 'as the ice was driving down upon us, it was evident, that if we remained longer between it and the land, it would drive us ashore, unless it should happen to take the ground before us.' Captain Cook on this, as on many other occasions, accommodated his views to the circumstances present, that there might be no unprofitable expenditure of time; and it may be said that in all his changes of plan, his measures were so directly adapted to his purpose, that without other communication his intentions were imme-

diately comprehended. The month of August was at this time far advanced ; and to make the most of the short remainder of the season, Captain Cook stood on westward for the coast of Asia, keeping in as high a latitude as the ice would permit. On the north side of his track were extensive bodies of ice, such as we call field ice. These generally are accumulations of loose floating pieces, which have been brought together by the wind blowing a length of time in one direction towards a coast. When the ice is so driven to land, it is evident that the inner pieces only take the ground ; the rest are confined by the wind, and when a change in the wind afterwards sets the ice from the land, it will preserve a position parallel to that which the coast gave it, until the strength and variety of winds have time to disperse it.

The deepest soundings we had in all this sea did not exceed thirty fathoms ; and this depth was found in latitude $68^{\circ} 45'$, midway between the coast of Asia and the coast of America. Northward, beyond that latitude, the soundings were observed to decrease : and in our run from the coast of America westward, we did not find the depth to increase, as is usual in running from land. Which peculiarities made us conclude, that there was land at no great distance from us to the north, and that we were sailing on a line parallel with its coast. Northward of our track also, as we ran towards the Asiatic coast, was a continuity of ice which seemed as if formed into a close barrier by a long extent of coast.

The nature of the soundings, with the absence of tide, gave to this sea so much the character of a mediterranean sea, that some on board, in particular Mr. BAILEY the astronomer, and myself, who being in the same ship communed

on the subject, were strongly of opinion that we were inclosed by land to the north, and that Asia and America were there joined; but we dared not venture to call in question the authority of MULLER.

If it is asked, whence then can come the great quantity of ice which is found in this sea? an answer readily presents itself. It is known that the Icy sea is frozen over every winter; and the northern part of this sea also has been known to be frozen over early in the winter. When the return of summer breaks up the ice, it will of course fill the sea with broken pieces.

Since the voyage of Captain Cook, little has been done towards ascertaining the termination of Asia. Commodore BILLINGS, an Englishman in the service of Russia, was employed to command an expedition for this express purpose, furnished with every assistance that could be devised towards ensuring success. His instructions directed him to make his departure from the river Kolyma, and to endeavour to follow the coast thence eastward by sea; with this additional instruction, 'that if coasting by sea should be found impracticable, and the information obtained on the spot should give hopes of effecting the purpose by land, he was then to endeavour to trace the coast by going in the winter in sledges over the ice.' Kossaks who had before been in the Tschuktzki country were selected to accompany Commodore BILLINGS, and among them was one who was the son of a Tschuktzki woman. 'Make agreement with them,' said the Instructions, 'or without agreement pay them the double of what is allowed to people who serve at sea. And whereas on a chart transmitted to us in the year 1764, a coast is marked

‘ opposite to the Kolyma, which stretches as a continuation
‘ of the continent of America, it will be of use if you can
‘ survey and describe the circumstances of that land.’

The history of Commodore BILLINGS’s expedition may be told in few words. He sailed with two light vessels out of the Kolyma, on the 24th of June, 1787. He met with much ice, and on the 20th of July, without having reached so far eastward as the island Sabedei, he relinquished the farther prosecution of the attempt by sea; at the very season, in fact, which was the most proper that could have been chosen for his outset from the Kolyma.* In this short attempt, Commodore BILLINGS did not even get sight of the north land; but an approach to it was to be inferred from the soundings. The snow and ice were at this time rapidly dissolving, so as to cause currents to set for several days continuance in one direction; and during that time, the water on the surface of the sea was so fresh as to be used for cooking, and sometimes for drinking.

Afterwards, Commodore BILLINGS, with the consent of the Tschuktzki people, made a progress by land along a part of the Tschuktzki coast. Most unaccountably, he chose for his point of outset for this journey, the bay of Saint Lawrence, which is on the south side of Bering’s Strait. He landed in the month of August with a party consisting of twelve persons, and travelled northward, keeping near the coast as far as to a bay called Klutchenie, which is at the extreme part of the Asiatic coast seen in Captain Cook’s voyage. By this time, winter had set in, and the sea was frozen over. The

* His lieutenant (the present Admiral Saretcheff) proposed and offered to proceed north-eastward in a light boat; but his offer was not accepted.

season proved a severe one; the cold was extreme, and the whole party had already been so much fatigued and harassed with their journey from the bay of St. Lawrence, that they were unable to pursue the coast farther northward. They afterwards, in their route westward towards the Kolyma, crossed a river, which, according to information from the Tschuktzki people, discharged itself into the sea seventy versts more north than the bay of Klutchenie.

In all this uncertainty respecting the north-east termination of Asia, the particular most worthy notice is, that the Tschuktzki people themselves do not appear, from any of the accounts which have been published, to know the extent of their country to the north, or to be able to give any satisfactory information concerning it, though it is known that some of their nation have travelled from the continent to islands in the Icy sea. The charts of the present century, which have assumed to give a limitation to Asia, differ a degree in the latitude of their northernmost cape.

It does not in the smallest degree detract from the merit or fame of the first discoverers, to question their having navigated round the north-east of Asia. Whether they sailed round a promontory, or crossed an isthmus, they are equally entitled to the honour of having first discovered for their countrymen the sea east of Kamtschatka. The most probable chance of completing the discovery, or of arriving at any certainty concerning a north-eastern boundary of Asia, is doubtless that which was recommended by the Russian admiralty to Commodore BILLINGS; i. e. to trace the coast in sledges when the sea is frozen.

The principal argument, and it is not a weak one, against

the probability of Asia and America being joined, is, that northern land in the Icy sea has repeatedly been supposed, and reported, to be an extension of the American continent; and it does not appear in any of the accounts to have been reported, or supposed, to join the Tschuktzki country. In Captain KRUSENSTERN'S memoir on the lands in the Icy sea, it is related, that very lately was explored an extent of 250 versts of coast of a northern land, which has been named the New Siberia. At the easternmost part which was seen of this land, the coast was observed to take a direction to the north-west. This direction of the coast might keep at a distance the supposition that it joined the Tschuktzki land: nevertheless, the coast may, and is supposed by the Russian discoverer, M. HEDERSTROOM, to turn afterwards to the east; for he gives it as his opinion that the New Siberia is a prolongation of America.

The Tschuktzki people would not explore farther north than afforded a prospect of reward for their pains. This, it is seen, has led them to some of the islands in the Icy sea; but no marks are noticed of their having been to the New Siberia.

The times for making expeditions of discovery in the Icy sea has generally been predetermined; but it would be more conducive to success to watch for favourable seasons. The state of the surface of the sea, when frozen, has also been found subject to much variation, depending upon the strength of the wind when the sea begins to be frozen. If in a calm, the surface will be smooth; if in boisterous weather, it will be rugged and bad for travelling.

III. *Additional facts respecting the fossil remains of an animal, on the subject of which two papers have been printed in the Philosophical Transactions, shewing that the bones of the sternum resemble those of the ornithorhynchus paradoxus. By Sir EVERARD HOME, Bart. V. P. R. S.*

Read January 22, 1818.

MY first account of the fossil bones of this most extraordinary animal attracted the notice of geologists, and collectors of extraneous fossils, and led Mr. JOHNSTON of Bristol, and the Revd. Mr. BUCKLAND of Oxford, to assist me with the materials in their possession, to make a farther progress in the description of its skeleton. An account of these specimens formed the substance of my second paper.

Since that time I have frequently communicated with these gentlemen, also with the Rev. PETER HAWKER of Woodchester rectory, Minchinhampton, and Dr. CARPENTER of Lyme; and have received from them many specimens I had not seen before, some of which it was difficult to determine to what part of the skeleton they belonged: but that being ascertained, and a similarity discovered to bones of the ornithorhynchus paradoxus, that circumstance alone made them, in my opinion, of sufficient importance to become the subject of a third communication to this Society.

There is also another reason for bringing forward these facts, and for doing so without any unnecessary delay; for, as my former papers were the means by which I acquired them, their being made known to the public, may lead those

gentlemen who have opportunities of examining the cliffs in which the bones are found, to renew their labours, and assist in making out all the essential parts of the skeleton.

In the description I am to give of the bones received, I shall begin with one, a part of which is shown lying on the scapula in Mr. BULLOCK's specimen, engraved in the first Paper; it was then taken for the portion of a rib accidentally brought there, but it is now found to have been nearly in its natural situation. It bears a resemblance to the clavicular bone in birds. In the annexed engraving (Pl. II. Fig. 1.) it is shown in its relative situation to the other bones.

The bones of which the sternum is composed, are the next to be taken notice of: this part of the skeleton was first pointed out by my friend Mr. BUCKLAND, who had visited every collection in which bones of this animal were known to be preserved; he met with several specimens in which two flat bones were united together, and their union covered by a bone not unlike the upper bone of the sternum in quadrupeds, which made him believe that all the three formed part of the sternum. These different specimens, at his request, were sent to London for my inspection, and Mr. BUCKLAND's suggestion proves to be right.

This discovery of the sternum destroys the analogy between this animal and the cartilaginous fishes, which, while the materials were more scanty, I had been led to suspect, in consequence of the bones of the pectoral fin of the squalus having a greater degree of correspondence to the pectoral fin or paddle of the fossil animal, than any other bones I have examined.

As this form of the sternum appeared at the time quite new, I was very anxious not to fall into an error, and was re-examining, with Mr. CLIFT, the different specimens, when it struck him that there was something similar to this mechanism in the sternum of the *ornithorhynchus paradoxus*: this remark led to a comparison of the bones, and they were found to have a general agreement that could not have been expected.

In the *ornithorhynchus*, the first bone of the sternum, at its upper end, has two lateral processes, which are connected with a similar process from each of the scapulæ; underneath the first bone are two flat bones united together, which union is covered by the first bone. On the outer edge of these flat bones, there is a broad process continued down from the scapula; in this process is the hollow to which the first bone or os humeri of the pectoral fin is articulated.

It will be seen that the difference between these bones of the sternum in the fossil skeleton and that of the *ornithorhynchus*, consists in the fossil skeleton having a clavicular bone, which is wanting in the other; and the *ornithorhynchus* having a long process from the scapula, in which is the cavity of the shoulder joint, wanting in the fossil skeleton. These slight differences are not readily perceived in looking at the parts, as will be seen on inspecting the annexed engravings, Pl. II. Fig. 1, and 2.

The mechanism which has been described, gives a very unusual surface for the muscles attached to the sternum, which move the first bone of the pectoral fin; and upon examining them in the *ornithorhynchus*, I find there is not only

the great pectoral muscle, going from the first bone of the sternum to the first bone of the pectoral fin, and a small one under it which may be called the small pectoral muscle ; but two large muscles, which have their origin from the flat bones and go to the first bone of the pectoral fin, or os humeri, and are inserted just below its head, that part being unusually broad, to allow of sufficient surface for their attachment. These muscles must be considered as peculiar to those animals that have this particular form of sternum.

As these muscles probably belonged to the animal whose fossil bones are under consideration, as well as to the ornithorhynchus, I have given them a particular description.

Till the sternum was discovered, it could not be ascertained in what manner the animal breathed ; and the ribs being attached to the middle part of the vertebræ, as in fishes, made me lean to the opinion that it breathed water ; but I now find, on more attentive consideration, that there is a difference between the mode of attachment of the ribs in this animal and in fishes, which admits of their having the motion of elevation and depression required in breathing air. In fishes, the attachment is single ; but here, as is shown in the engravings in a former paper, it is double ; and the two are considerably apart, as in the bird. Till it was determined whether the animal breathed air or water, little attention was paid to the nostrils, further than to find their place on the skull ; more particularly as in the only head I had seen, the bony scales of the opposite eye had been pushed through an aperture situated where I considered the opening of the nostril must have been ; but I

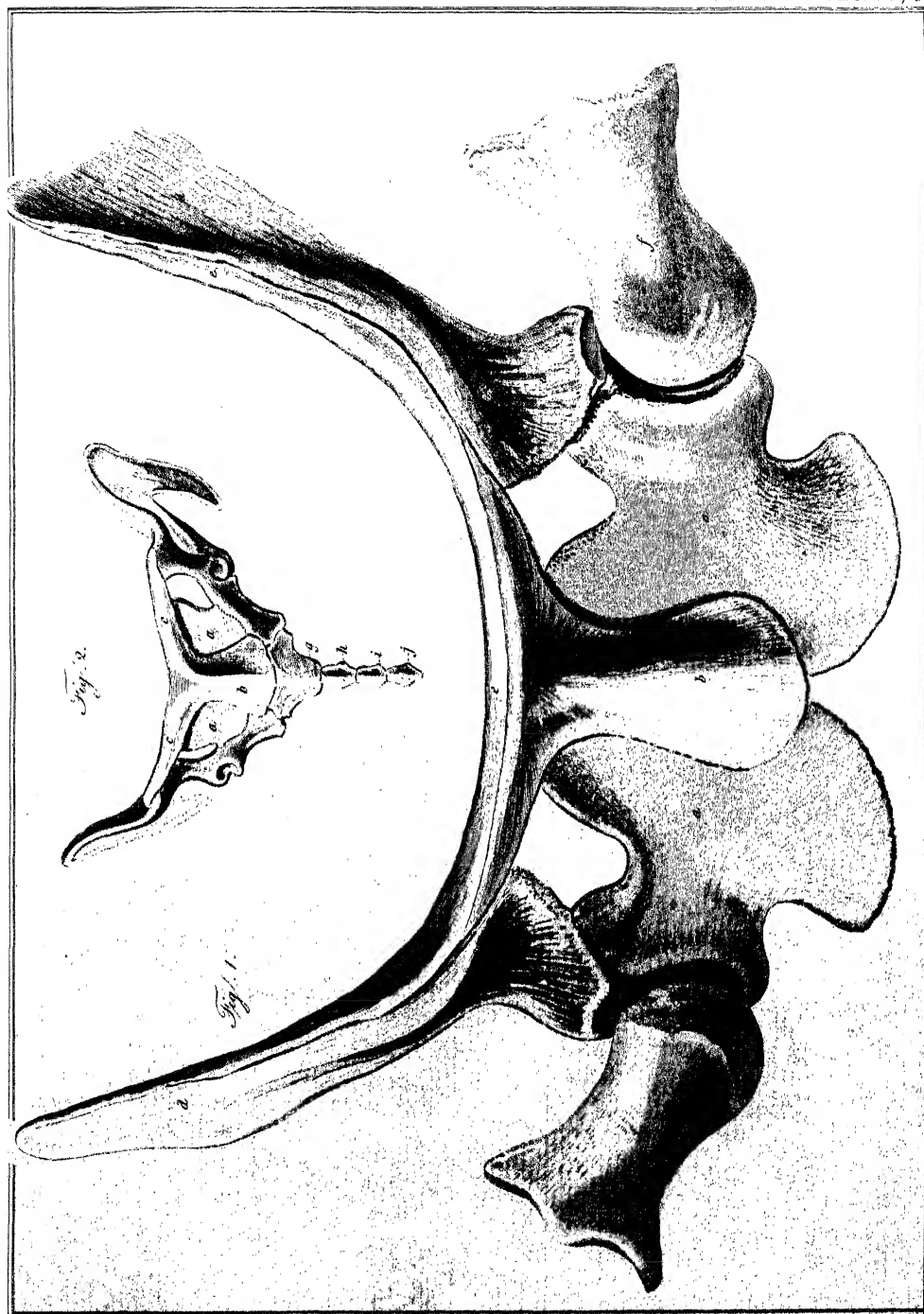
dd The two scapulæ which differ from those in Fig. 1. by extending below the two flat bones, and forming the whole of the glenoidal cavity of the shoulder.

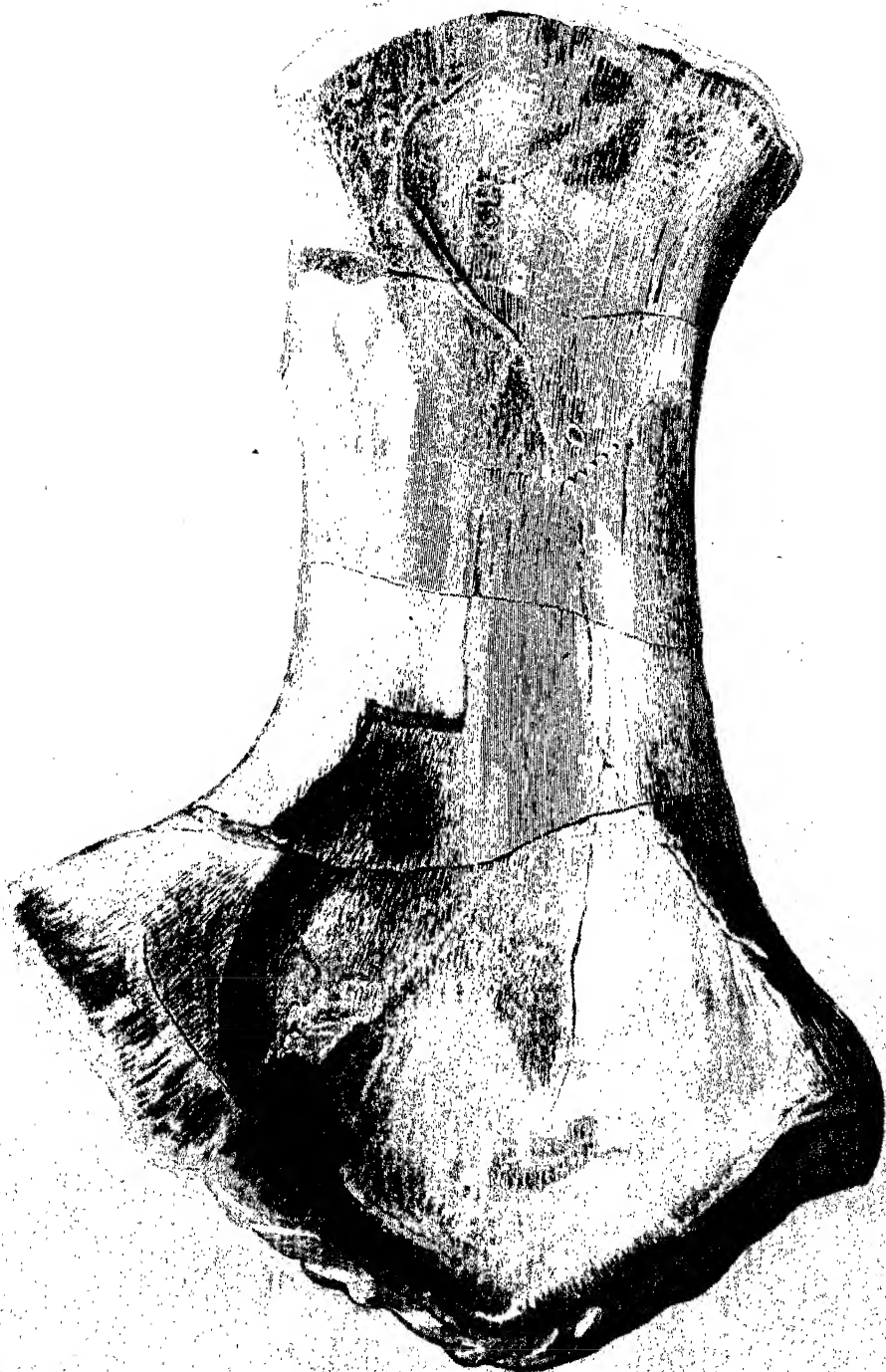
ghij Four bones belonging to the sternum.

All the parts are of the natural size.

PLATE III.

The representation of a fossil bone of the natural size, which appears to belong to the same animal. Its place in the skeleton cannot at present be ascertained, since it is not yet known whether in this animal there is a regular pelvis. It bears a greater resemblance to the first bone of the paddle than to any other; so that if the animal has a posterior paddle, this must belong to it.





IV. *An Account of experiments for determining the length of the Pendulum vibrating seconds in the latitude of London. By Capt. Henry Kater, F. R. S.*

Read January 29, 1818.

To determine the distance between the point of suspension and centre of oscillation of a pendulum vibrating seconds in a given latitude, has long been a desideratum in science. Many experiments have been made for this purpose, but the attention of all who have hitherto engaged in the enquiry (excepting WHITEHURST) appears to have been directed to the discovery of the centre of oscillation. The solution of this problem depending, however, on the uniform density and known figure of the body employed, (requisites difficult if not impossible to be ensured in practice,) it is not surprising that the experiments made by different persons should have been productive of various results.

When I had the honour of being appointed one of the committee of the Royal Society* for the investigation of this interesting subject, I imagined that the least objectionable mode of proceeding would be to employ a rod drawn as a wire, in which, supposing it to be of equal density and diameter throughout, the centre of oscillation, as it is well known, would be very nearly at the distance of two-thirds of the length of the rod from the point of suspension; and I purposed by inverting the rod, and taking a mean of the results in each position, to obviate any error which might

arise from a want of uniformity in density or figure. After numerous trials however, and as frequent disappointments, I was at length convinced of the impracticability of obtaining a rod sufficiently uniform, and I was besides aware, that under certain circumstances errors might arise from this cause which it would be impossible by any method to detect.

Not feeling at all satisfied with the prospect which the use of a rod presented, I endeavoured to discover some property of the pendulum of which I might avail myself with greater probability of success; and I was so fortunate as to perceive one, which promised an unexceptionable result. It is known that the centres of suspension and oscillation are reciprocal; or in other words, that if a body be suspended by its centre of oscillation, its former point of suspension becomes the centre of oscillation, and the vibrations in both positions will be performed in equal times. Now the distance of the centre of oscillation from the point of suspension, depending on the *figure* of the body employed, if the arrangement of its particles be changed, the place of the centre of oscillation will also suffer a change. Suppose then a body to be furnished with a point of suspension, and another point on which it may vibrate, to be fixed as nearly as can be estimated in the centre of oscillation, and in a line with the point of suspension and centre of gravity. If the vibrations in each position should not be equal in equal times, they may readily be made so, by shifting a moveable weight, with which the body is to be furnished, in a line between the centres of suspension and oscillation; when the distance between the two points about which the vibrations were performed being measured, the

length of a simple pendulum, and the time of its vibration will at once be known, uninfluenced by any irregularity of density or figure.*

An unexceptionable principle being thus adopted for the construction of the pendulum, it became of considerable importance to select a mode of suspension equally free from objection. Diamond points, spheres, and the knife edge, were each considered ; but as it was found difficult to procure diamond points sufficiently well executed, the knife edge was preferred, after many experiments had been made with

* In the *Connoissance des Temps* for 1820, is an article by M. de PRONY on a new method of regulating clocks. At the conclusion of this article is a short note, in which the author adds, " J'ai proposé en 1790 à l'Académie des Sciences un moyen " de déterminer la longueur du pendule en faisant osciller un pendule composé sur " deux ou trois axes attachés à ce corps. (voyez mes Leçons de Mécanique, art. " 1107 et suivans) Il paroît qu'on a fait ou qu'on va faire usage de ce moyen en " Angleterre." On referring to the *Leçons de Mécanique*, as directed, I can perceive no hint whatever of the possibility of determining the length of the seconds pendulum by means of a compound pendulum vibrating on *two* axes, but it appears that the method of M. de PRONY consists in employing a compound pendulum having *three* fixed axes of suspension, the distances between which, and the time of vibration upon each, being known, the length of three simple equivalent pendulums may thence be calculated by means of formulæ given for that purpose. M. de PRONY indeed proposes employing the theorem of HUYGENS, of which I have availed myself, of the reciprocity of the axis of suspension and that of oscillation, as *one* amongst other means of simplifying his formulæ, and says, " J'ai indiqué les moyens " de concilier avec la condition à laquelle se rapportent ces formules, celle de rendre " l'axe moyen le reciproque de l'un des axes extrêmes ; J'emploie pour les ajustemens " qu'exigent ces diverses conditions un poids curseur dont j'ai exposé les propriétés " dans un mémoire publié avec la *Connoissance des Temps* de 1817." Now it appears evident from this passage, that M. de PRONY viewed the theorem of HUYGENS *solely* with reference to the simplification of his formulæ ; for had he perceived that he might thence have obtained *at once* the length of the pendulum without further calculation, the inevitable conclusion must instantly have followed that his third axis and his formulæ were wholly unnecessary.

spheres, the result of which it may not be useless for a moment to dwell upon.

It is known, that if two curved surfaces be ground together in every possible direction, they will become portions of spheres; and thus a perfect sphere may be formed by grinding a ball in a hemispherical cup. If a pendulum vibrate on such a sphere, working in a conical aperture, it is evident that the centre of the sphere will be accurately in the axis of vibration. In trying this method, however, it was found, that the friction was so considerable, as to bring the pendulum to a state of rest after a few vibrations; and when the friction was sufficiently diminished, by a contrivance which it is unnecessary to describe, the *lateral* force of the pendulum in an arc of two degrees and a half, was sufficiently powerful to carry the ball entirely out of the socket; and it was consequently evident, that though the arc of vibration might not be large enough to effect this, it must necessarily cause the ball in some degree to ascend the inclined plane of the aperture; and this consideration induced me to abandon at once a mode of suspension which I should otherwise have esteemed the best that could have been employed.

The principal objections to the use of a knife edge, appeared to be, the difficulty of forming it perfectly straight, and the possibility that it might suffer a change of figure from wear, during the experiments, which might introduce an error not to be detected. The first of these objections I found to be perfectly groundless, as a knife edge can be made so as not to deviate sensibly from a right line. The second objection would indeed be of weight, were the usual method of determining the time of vibration resorted to, by coin-

paring the pendulum with a clock, at the distant intervals of 24 hours ; but it will hereafter appear, that should any alteration in the form of the knife edge take place, it must become perceptible every ninth minute ; in addition to which, I proposed to measure the distance of the knife edges both before and after the experiments, when any change would of course be immediately detected.

Description of the pendulum employed.

The pendulum constructed upon these principles is formed of a bar of plate brass, one inch and a half wide, and one eighth of an inch thick. Through this bar, two triangular holes are made, at the distance of 39.4 inches from each other, to admit the knife edges. Four strong knees of hammered brass of the same width as the bar, six inches long, and three quarters of an inch thick, are firmly screwed by pairs to each end of the bar, in such a manner, that when the knife edges are passed through the triangular apertures, their backs may bear steadily against the perfectly plane surfaces of the brass knees, which are formed as nearly as possible at right angles to the bar. The bar is cut of such a length, that its ends may be short of the extremities of the knee pieces about two inches.

Two slips of deal 17 inches long, and of the same thickness as the bar, are inserted in the spaces thus left between the knee pieces, and are firmly secured there by pins and screws. These slips of deal are only half the width of the bar ; they are stained black, and in the extremity of each, a small whalebone point is inserted for the purpose of indicating the extent of the arc of vibration.

A cylindrical weight of brass, three inches and a half diameter, one inch and a quarter thick, and weighing about two pounds seven ounces, has a rectangular opening in the direction of its diameter, to admit the knee pieces of one end of the pendulum. This weight being passed on the pendulum, is so thoroughly secured there by means of a conical pin fitting an opening made through the weight and knee pieces, as to render any change of position impossible. A second weight of about seven ounces and a half, is made to slide on the bar near the knife edge at the opposite end; and this weight may be fixed at any distance on the bar by two screws with which it is furnished.

A third weight, or rather slider of only four ounces, is moveable along the bar; and is capable of nice adjustment by means of a screw fixed to a clamp, which clamp is included in the weight. This slider is intended to move near the centre of the bar. It has an opening, through which may be seen divisions, each equal to one twentieth of an inch, engraved on the bar; and a line is drawn on the edge of the opening to serve as an index to determine the distance of the slider from the middle of the bar.

We now come to the most important part, the knife edges. These are made of that kind of steel which is prepared in India, and known by the name of wootz. Their form is triangular, and their length one inch and three quarters. Mr. STODARD was so obliging as to forge them for me: they were made as hard as possible, and tempered by immersing them merely in boiling water.

The knife edges were ground on a plane tool, which necessarily ensured a perfectly straight edge. This was ascer-

tained by bringing the edge of the one in contact with the plane of the other, when, if no light was perceptible between them in any position, it was inferred that the edge was a right line. They were then carefully finished on a plane green hone, giving them such an inclination as to make the angle on which the vibrations are performed about 120 degrees.

Previously to the knife edges being hardened, each was tapped half way through, near the extremities, to receive two screws, which being passed through the knee pieces, drew the knife edges into close contact with them, the surfaces of both having been previously ground together to guard against any strain which might injure their figure. A plan of the pendulum is given in Plate IV. Fig. 1.

The support, and other apparatus.

The support of the pendulum is represented in Plate IV, Fig. 2. It consists of a piece of bell metal six inches long, three inches wide, and three eighths of an inch thick. An opening is made longitudinally through half the length of the piece, to admit the pendulum, and the bell metal is cast with a rectangular elevation on each side of the opening extending the whole length of the piece. Two plates of agate* were cemented to this elevated part, beds having been made to receive them, in order that their surfaces might be in the same plane with that of the bell metal. The whole was then ground perfectly flat. A frame of brass represented at Fig. 3, is attached by two opposite screws, which

* Plates of hard steel were first tried, but were found to have suffered penetration by the knife edge.

serve as centres, to the sides of the elevated part of the support, and one end of this frame being raised or depressed by means of the screw A, the pendulum when placed with its knife edge resting in Ys, at the other end of the frame, could be elevated entirely above the surface of the agate, or be gently lowered until the knife edge rested wholly upon it, and thus the knife edge was sure to bear always precisely on the same part of the agate plane, by elevating the Ys, above its surface, placing the knife edge in them, and then letting down the whole gently by means of the screw, till the Ys were completely clear of the knife edge. The support was firmly screwed to a plank which will hereafter be described.

To the kindness of HENRY BROWNE, Esq. F. R. S., I am essentially indebted for the success of the experiments which form the subject of this paper. He most obligingly allowed me the use of his house, his excellent time-pieces, and transit instrument, assisting me with indefatigable zeal by his very accurate daily observations, and intermediate comparisons for determining the rate of the clock. The house is substantially built, and is situated in a part of Portland Place not liable to much disturbance from the passing of carriages. The room in which the experiments were made is the last of two on the ground floor, communicating with each other and facing the north. The temperature consequently is very steady, and if necessary, may be raised to any given degree by a fire in the first room. The clock with which the pendulum was compared was made by ARNOLD; and in addition to the gridiron compensation for temperature, its pendulum is suspended by a spring, the strength of which is so adjusted, that the vibrations in different arcs are performed in equal times. This

clock is firmly screwed to the wall, in a recess opposite to the window. Near to this, on the wall which is at right angles to the recess, is fixed another time-piece by CUMMING, which was the property of the late General ROY, and is considered by Mr. BROWNE to be the best in his possession. Respecting this clock, it will be sufficient to remark, that three tenths of a second was the greatest variation in its daily rate from the 22d February, when the observations commenced, to the 31st July; and consequently the deviation from its mean rate during that period, did not exceed 0,15 of a second per day. This clock has been used as the standard of comparison, the time having been taken from the transit instrument by a chronometer of ARNOLD's. With such advantages it will be confessed that there can be little chance of error arising from the rate of the clock.

A plank of well seasoned mahogany, two feet wide, and three inches thick, was forcibly driven between the walls forming the sides of the recess, until it was near the top of the clock case. To this the support of the pendulum before described was firmly screwed, and carefully levelled, in such a position as to allow the pendulum to vibrate as near as possible to the clock case without touching it; and that when at rest, it might appear to an observer in front of the clock, to pass over the centre of the dial plate, its extremity reaching a little below the centre of the ball of the pendulum. Beneath, fixed to the clock case, was an arc divided into degrees and tenths, to determine the extent of the vibrations. Such a portion of the plank was cut away as was necessary to admit of the pendulum being placed on its support. A circular white disk was pasted on a piece of black paper,

which was attached to the ball of the pendulum of the clock ; and this disk was of such a diameter as when both pendulums were at rest, to be just hid from an observer at the opposite side of the room, by one of the slips of deal which form the extremities of the brass pendulum.

Though there was little reason to imagine that the vibrations of the pendulum could communicate any motion to a support so firm as that which has been described, it became a point of considerable importance to verify this by actual experiment. For this purpose I had recourse to a delicate and simple instrument invented by Mr. HARDY, clock-maker, the sensibility of which is such, that had the slightest motion taken place in the support, it must have been instantly detected. This little instrument is represented in Plate IV. Fig. 4. It consists of a steel wire, the lower part of which inserted in the piece of brass which serves as its support, is flattened so as to form a delicate spring. On the wire, a small weight slides, by means of which it may be made to vibrate in the same time as the pendulum to which it is to be applied as a test. When thus adjusted, it is placed on the material to which the pendulum is attached ; and should this not be perfectly firm, its motion will be communicated to the wire, which in a little time will accompany the pendulum in its vibrations. This ingenious contrivance appeared fully adequate to the purpose for which it was employed, and afforded a satisfactory proof of the stability of the point of suspension.

A firm triangular wooden stand, as high as the ball of the pendulum, was screwed to the floor at the distance of nine feet in front of the clock. This served as a support, to which was attached a small telescope, magnifying about four times,

which was capable of a horizontal motion on its axis, a vertical motion, and a motion at right angles to the line of sight. In the focus of the eye-glass was a diaphragm forming a perpendicular opening, the sides of which were parallel, and capable of being placed nearer, or further asunder. The edges of this diaphragm were adjusted so as to form tangents to the horizontal diameter of the white disk, and consequently to coincide with the edges of the slip of deal. When, therefore, both pendulums were at rest, nothing was visible through the telescope, excepting the divided arc for ascertaining the extent of the vibrations, and which was seen through a horizontal opening made for that purpose in the top of the diaphragm.

Method of determining the number of vibrations made by the pendulum in 24 hours.

If both pendulums be now set in motion, the brass pendulum a little preceding that of the clock, the following appearances may be remarked. The slip of deal will first pass through the field of view of the telescope at each vibration, and will be followed by the white disk. But the distance between the centres of suspension and oscillation in the brass pendulum being rather the longer, the pendulum of the clock will gain upon it, the white disk will gradually approach the slip of deal, and at length, at a certain vibration, will be wholly concealed by it. The minute and second at which this total disappearance is observed, must be noted. The pendulums will now be seen to separate, and after a time will again approach each other, when the same phenomenon will take place. The *interval* between the two coincidences in seconds, will give the number of vibrations made by the pendulum of the clock; and the number of oscillations of the

brass pendulum, in the same interval, may be known by considering that it must have made two oscillations less than the pendulum of the clock. Hence by simple proportion, as the vibrations made by the pendulum of the clock, are to the number of vibrations made by the brass pendulum, so are the vibrations made by the pendulum of the clock in 24 hours, to those of the brass pendulum in the same period.*

Many experiments were made in order to select such a distance of the knife edges as might give an interval which would allow of the determination of the time of coincidence without an error of a single second,† and yet afford a convenient number of intervals before it should become necessary to renew the motion of the pendulum. At the first coincidence, the velocity of the brass pendulum, at the lowest part of the arc, must not exceed that of the pendulum of the clock, otherwise the disk would disappear for an imperceptible time, and then re-appear; and this limits the extent of the arc of vibration.

Again; the observations must not be continued beyond a certain diminution of the arc of vibration, otherwise the *space*, which the pendulum of the clock has to gain on the brass pendulum in one vibration, becomes so small as to render the observation of the time of coincidence in some degree uncertain; and, should the *space* be so far diminished as to be less than the error or deviation from a right line, which would

* In order to render the calculation more easy, the clock has always been supposed to keep mean time, or to make 86400 vibrations in 24 hours, and the variation from this number, or the rate of the clock (being a very small quantity) has been afterwards applied as a correction.

† The principle on which this method of coincidences is founded, was employed by Dr. WOLLASTON, in May 1808, in some experiments in which he was then engaged, the moment of coincidence being determined however by sound instead of sight.

probably take place in the adjustment of the sides of the diaphragm, the end of the pendulum, and the disk, the results would be erroneous, as the interval would go on increasing till the pendulum came to a state of rest.

The interval which best fulfilled these conditions was found to be about 530 seconds. This admitted five coincidences (affording four intervals) to be taken before the arc became too small for the observations to be continued with safety. With this interval an error of one second in the time of coincidence would occasion an error of only 0,63 in the number of vibrations in 24 hours.

Here it must be evident that no sensible alteration could take place in the knife edge during the experiments without its becoming perceptible at every coincidence, since the number of vibrations in 24 hours deduced from each interval, must vary with any change in the form of the knife edge.

The following was the method pursued in making the observations. The small weight or slider being placed with its index at a certain distance (say one inch and a half) from the middle of the pendulum towards the great weight, and the second weight about five inches from the knife edge, the Ys of the support were elevated, the knife edge of the pendulum was placed in them, with the great weight *above*, and the frame gently lowered till the knife edge was left on the surface of the agate. The requisite adjustments of the telescope having been made, the pendulum was set in motion in an arc not exceeding one degree and four tenths, in order that its velocity might not be greater than that of the pendulum of the clock.

The minute and second, at which the disk ceased to be visible, was then carefully noted; and the arc of vibration seen through the telescope, the height of the barometer, and the temperature indicated by a thermometer suspended on the clock case near the middle of the brass pendulum, were also observed and registered. Five successive coincidences were thus taken, and the number of vibrations in 24 hours was deduced from them in the manner before described; but the vibrations thus obtained being made in different arcs, it became necessary to apply a correction to determine what they would have been in an arc infinitely small. For this correction I might have used a formula depending on the decrease of the arcs in geometrical progression, whilst the times decrease in arithmetical; but as there is an uncertainty in observing the arc of vibration amounting to one or two hundredths of a degree, this method, though more perfect in theory, would have been an unnecessary refinement in practice.

The error arising from the greater length of the vibration in a circular arc, being nearly as the square of the arc, if the mean of the observed arcs at the commencement and end of each interval be taken, and its square multiplied by 1.635 (the difference between the number of vibrations made by the pendulum in 24 hours, in a cycloid and in an arc of one degree,) the required correction will be obtained, to be added to the number of vibrations before computed.

The mean of these last results being taken, and also the mean of the observed temperatures at the first and last coincidences, the number of vibrations in 24 hours was obtained

at a certain temperature, and altitude of the barometer, in an infinitely small arc, the great weight being *above*.

The frame of the support was now elevated, the pendulum was inverted, placed in the Ys, with the great weight *below*, and the knife edges being gently let down as before on the agate plane, the same process with respect to the observations was followed, which has just been described. And if the mean temperature differed from that in the former position of the pendulum, the mean number of vibrations was corrected for such difference of temperature, the expansion of the pendulum being known by experiments hereafter to be detailed, and consequently the gain or loss in 24 hours by a given change of temperature.

The mean number of vibrations thus found, differing from that given in the former position of the pendulum, the second weight was moved, the number of vibrations again determined, and the pendulum being inverted, the process was repeated until the vibrations in 24 hours, in either position of the pendulum were brought as near to an equality as could readily be effected by means of this weight; it was then firmly secured in its place.

Whatever alteration may be made in the arrangement of the weights, the effect on the vibrations (except in one particular instance) will be the same in both positions of the pendulum, always increasing or diminishing their number in both cases, though in different degrees; and, the vibrations will be least affected by such change when the great weight is below, and will consequently be nearest to the truth in this position. No doubt, therefore, can arise, as to the kind of correction required. The number of

vibrations after the adjustment by the second weight has been completed, must be left *in defect*, for a reason which will be immediately apparent.

There is a point in the pendulum where the effect of the slider in increasing the number of vibrations is a maximum; and it appears from Dr. YOUNG's investigations, that this point in one position of the pendulum is different from that in the other. *Very near* either of these points, the pendulum being in its corresponding position, the motion of the slider produces scarcely any change in the number of vibrations; but the slider being then more distant from the point of maximum belonging to the other position of the pendulum, the corresponding increase of the number of vibrations arising from such motion of the slider, will, in that position be very perceptible.

In the present instance, the point of maximum in either position of the pendulum, is about four tenths of an inch below the middle, and consequently the distance of the two points from each other, is about eight tenths of an inch. The slider which had remained stationary during the adjustment of the second weight at about one inch and a half from the middle of the pendulum towards the great weight, must now be shifted, (say one inch) towards the middle of the pendulum, in order to increase the number of vibrations which it may be recollected were left *in defect*, so that they may be in *excess*. It is evident that the true number of vibrations will be found, when the slider is somewhere between its first and second position. Let the slider be now placed half-way between these two points. If the number of vibrations in this

third position be still in excess, the truth will lie between the first and third positions of the slider. And thus by continually bisecting with the slider, the distance of the two last found points, the number of vibrations when the great weight is *below*, will rapidly approach the truth, being alternately in defect and in excess; and when the approximation is such as that the difference in either position of the pendulum becomes inconsiderable, the vibrations, when the great weight is below, may be taken for the truth; and thus the number of vibrations in 24 hours, of a pendulum equal in length to the distance between the knife edges, will be known at a certain temperature, and at an observed height of the barometer. The general arrangement of the apparatus is represented by Plate V.

Of the apparatus and methods employed for the measurement of the distance between the knife edges, and for the comparison of the British standard measures of the highest authority.

The microscopes used for this purpose, were made by Mr. THOMAS JONES, of Cockspur-street. They are both furnished with cross threads of spider's web, as well as with a single thread for the purpose of bisecting a dot if required, and are in other respects of a similar construction with those described by Sir GEORGE SHUCKBURGH EVELYN, in the Philosophical Transactions for 1798, but are more powerful, and the micrometer is capable of far greater precision.

The object glass of the micrometer microscope is of one inch focus, the distance from the object glass to the spider's threads 3.25 inches, the focus of the compound eye-glass rather less than one inch, the magnifying power 18 times.

In the other microscope, which I shall call the fixed microscope, the object glass is of three quarters of an inch focus, and the magnifying power consequently greater. The micrometer head is divided into one hundred parts.

Each microscope slides in a tube, which is fixed in a plate of brass forming part of its support; and this plate moves in a dovetail, by which the microscope may be brought over the object to be viewed, when it is firmly clamped by a screw.

A piece of well seasoned mahogany, four inches and three quarters, by three inches, served as a beam to which the supports of the microscopes were screwed, their centres being 39.4 inches asunder.

Two screws with milled heads, supported the extremities of the beam in front, and a piece projecting from the middle of the beam behind, served as a third leg. By means of the screws, the focus of either microscope could be nicely adjusted at pleasure, without any risk of altering their distance from each other.

My first object was to ascertain the degree of precision of which vision is capable when assisted by the microscope. For this purpose, a very fine line was drawn on a polished piece of brass, and the microscope being carefully adjusted so as to be free from parallax, by causing the image of the line to bisect the angles formed by the spider's threads, moving the eye to the right and left and remarking whether the image changed its situation, and if it did, varying the distance of the microscope from the object accordingly, until the line appeared stationary, the micrometer screw was turned back, and the spider's threads brought up again till the angle

formed by them appeared to be accurately bisected by the line. The division of the micrometer was then noted, and this was repeated several times with scarcely a sensible difference in the result; and thus I assured myself that no error worthy of remark was to be apprehended from imperfection of vision.

The next step was to determine the value of one division of the micrometer head. By the kind interest of Sir JOSEPH BANKS, I was favoured with the use of the standard scale which belonged to the late Sir GEORGE SHUCKBURGH EVELYN, and which is described in the Philosophical Transactions for 1798. This scale, the work of Mr. TROUGHTON, is second to none in the kingdom in point of accuracy of division, and is too well known to render any further remark necessary. The microscope being carefully adjusted for parallax, one inch, from the 39th to the 40th, was measured by successive tenths, and the mean taken as the value of one tenth of an inch. The measurement of the same inch was repeated ten times at different periods, the microscopes being previously adjusted anew each time for parallax. The mean results of such measurements are as follows.

Divisions of the micrometer to $\frac{1}{10}$ of an inch.

2335.00
2333.75
2337.55
2337.32
2334.50
2336.90
2335.75
2338.30
2335.85
2337.85

Mean 2336.277

Hence, the value of one division of the micrometer appears to be $\frac{1}{23363}$ of an inch.

In the course of these measurements, differences occurred for which I was at a loss to account; but at length it appeared that they were to be attributed to remaining parallax; for whatever care be taken in adjusting the microscope, it is scarcely possible to bring the image of the object precisely in the same plane with the threads, and the image will consequently be of various dimensions, according to its distance from this plane. Unless, therefore, the most minute attention be paid to the adjustment for parallax, the error arising from this cause will be considerable; and I may here remark that I believe the difficulty of bringing the image into the plane of the threads, to be the source of by far the most serious errors to which measurements by means of microscopes are liable.

I had now to examine the equality of the threads of the micrometer screw. For this purpose, two fine lines were drawn near each other on a piece of brass, and the micrometer being turned back as far as it would go, the distance of the lines was carefully measured; and this was repeated, proceeding through the whole length of the screw, always advancing the micrometer one revolution previous to each successive measurement. The result of this severe test will best appear by giving the numbers themselves.

Divisions of the micrometer.

502,0
 501,5
 501,0
 502,0
 501,5
 502,0
 502,0
 502,5
 502,0
 502,0
 502,5
 501,0
 501,5
 502,0
 501,0
 502,5
 501,0
 500,0
 500,0
 500,5

Mean 501,5

The mean is 501,5, and the greatest difference from the mean only one division, amounting to $\frac{1}{23363}$ of an inch, a degree of accuracy truly surprising, when it is considered that all errors of observation are included in this minute quantity.

Comparison of the different standards.

The microscopes being placed at the distance of 39,4 inches, were advanced by single tenths, from zero of the scale through the space of two inches, and the mean of twenty measurements thus obtained being compared with the distance from zero to 39,4 inches, this last was found to be

in defect 1,2 divisions of the micrometer, or ,00005 of an inch. And as this is the portion of the scale employed in ascertaining the distance between the knife edges, this difference must ultimately be subtracted to obtain the distance of the knife edges, in parts of the mean value of the scale.*

From the high importance which attaches to General Roy's scale, as having formed the basis of the Trigonometrical Survey of the kingdom, I was particularly desirous of comparing it with that of Sir G. SHUCKBURGH, in order that I might be enabled to give the length of the pendulum in parts of that standard which constitutes the foundation of one of the most important scientific operations ever carried on in this country. Fortunately, this scale was purchased at the sale of General Roy's effects by Mr. BROWNE, who readily confided it to my care. From the mean of a number of comparisons, I found the distance from zero to 39,4 of General Roy's scale, equal to 39,40144 of Sir G. SHUCKBURGH's standard.†

* From an examination of this scale by the late Sir G. SHUCKBURGH, it appears that the greatest liability to error is ,00033 of an inch, or as corrected by Mr. TROUGHTON, ,000165 of an inch, the chances against which are as 9 to 1.

† The very great difference between this result and that stated by Sir GEORGE SHUCKBURGH, in the Philosophical Transactions for 1798, renders it necessary for me briefly to detail the manner in which the comparisons were made. The two scales were placed in contact, and remained thus for twenty four hours; after which, sixteen comparisons were taken in the course of the day; but these were rejected in consequence of the temperature having increased six degrees during the operation. When the scales had been together forty-eight hours, sixteen other comparisons were made during two succeeding days, the thermometer remaining steadily at 70°. The greatest difference between any one of these last and the mean result, did not amount to four divisions of the micrometer. The mean of the first set of observations ex-

The standard yard made by BIRD in 1758, for the House of Commons, better known by the name of BIRD's Parliamentary standard, is little adapted for measurements where great precision is necessary. The yard is determined by two large dots made on gold pins, which are let into a bar of brass. The mean of a number of bisections of these dots gave their distance equal to 36,00016 inches of Sir GEORGE SHUCKBURGH's scale.

Measurement of the pendulum.

The pendulum was let into a solid piece of mahogany edgewise, to such a depth that the knife edges were about one twentieth of an inch above its surface. To one end of the pendulum, a common spring steelyard was attached by its hook, and a string being passed through the ring, and fastened to an upright piece of wood screwed to the end of the mahogany case, the pendulum was extended by a force rather greater than its own weight (about ten pounds), and consequently, no error (if any such were to be apprehended)

ceeded that of the last by ,00017 of an inch. Imagining that the difference between Sir GEORGE SHUCKBURGH's result and mine, might possibly be occasioned by an error in the divisions bounding that part of General ROY's scale which I had employed, I compared it with various other portions, and found no greater difference than might have been expected from unavoidable imperfection of division. It is to be presumed then, that the error into which Sir GEORGE SHUCKBURGH appears to have fallen, must have arisen from the two scales not having been of the same temperature at the time they were compared, particularly as Sir GEORGE SHUCKBURGH's is by far the most massive of the two. I may here add, that last winter wishing to know whether the expansion of the two scales was equal, I roughly compared them together once, at the temperature of 33°, when it appeared that 42 inches on General ROY's scale, was equal to about 42,001 inches of Sir GEORGE SHUCKBURGH's standard.

could arise from a difference in the length of the pendulum in its vertical and horizontal positions.

The knife edges were fixed as nearly as could be done by mechanical means, at right angles to the bar of the pendulum; but the bar being flexible, they would most probably, when the pendulum was extended for the purpose of measurement, be found to be not precisely parallel to each other, and would consequently require some adjustment. To effect this, two opposite screws were passed through the sides of the mahogany case, so as to act in a transverse direction against that extremity of the pendulum which was next the steelyard, and the microscopes being brought over the extreme points of the knife edges, alternately on either side of the bar, the requisite parallelism was readily obtained by means of the screws, sufficient room having been left in the mahogany case for the very small motion of the extremity of the pendulum which might be found necessary. This arrangement is represented in Plate IV. Fig. 5.

To obtain the distance between the knife edges, two different methods were used. For the first, four rectangular pieces of brass were prepared, about half an inch square. Very near to the perfectly straight edge of each, a fine line was drawn, to be viewed through the microscope, and these lines were each crossed at right angles by two others, intended to indicate that part of the first line from which the measurements were to be taken. These pieces were marked A, *a*, and B, *b*.

The pieces A and *a*, being placed with their edges in contact, in which position they were kept by the pressure of a spring, the distance between the fine lines first drawn was

carefully measured with the micrometer, and from a mean of eight observations, the greatest difference between which did not exceed one division, was found to be 329.09 divisions.

The same was done with the pieces B and b, and the distance of the lines from a mean of sixteen observations appeared to be 366.96 divisions.

The knife edges being adjusted as nearly as possible parallel to each other, the pieces A, a, and B, b, were placed in contact with those parts of the knife edges on either side of the bar, on which the vibrations were to be performed, and were retained in their places by the pressure of slight springs, attached to the mahogany case.

The microscopes were now brought over the pieces A and a, so as for the lines before described to bisect the cross threads, when the division of the micrometer was noted.

The same was done with the pieces B and b; and the division of the micrometer was also registered.

The pendulum being removed, the standard scale* was placed beneath the micrometer, and its zero being made to bisect the angles of the fixed microscope, the cross threads of the micrometer microscope were brought to 39.4 of the scale, and the revolutions and parts of the micrometer were noted.

From these data, and the respective distances of the lines on A and a, and on B and b, when the pieces were in contact, the distance of the knife edges on either side of the bar may be readily obtained, and the mean being taken, will obviously correct any error arising from a want of perfect parallelism in the knife edges.

It is very generally believed that measurements from a

* The scale constantly referred to, is Sir GEORGE SHUCKBURGH's standard.

knife edge, or from a line terminating a surface, are liable to much uncertainty from what has been called *irradiation*, or indistinctness of the image. But this is by no means the fact; for if the reflection of light from the knife edge be prevented, and it be viewed on a white ground, it may be made to bisect the cross threads of the microscope, with nearly the same precision as could be attained by the use of a line. There is, however, a correction necessary to be applied in this case, and I shall proceed to describe the method employed for ascertaining its amount.

A slip of writing-paper was pasted on the mahogany case, under each knife edge, extending beyond it about the tenth of an inch, and adjoining, was a piece of black paper to prevent the reflection of light on the knife edge from the surrounding objects. The knife edge now appeared through the microscope, as a well defined dark object on a white ground.

Marks were made on the paper close to the knife edges at equal distances on each side of the bar. These were intended to indicate those parts of the knife edges equally distant from the middle, from which the measurements were to be taken.

The knife edges being adjusted parallel to each other, in the manner before described, the microscopes were brought successively over such marks on the paper, as were at the same distance from the bar, and the mean of each pair of observations being referred to the scale, gave a distance of the knife edges free from any error which would be occasioned by a want of parallelism.

The knife edges bisecting the cross threads of the micro-

scopes, pieces of black paper were slid beneath them, when they appeared to start forwards towards each other, the images continuing perfectly sharp and well defined.

The distance between the knife edges appeared to be now considerably less than before ; and it remained to determine the difference, in order to apply its half, as a correction to the distance first obtained.

For this purpose, the reading of the micrometer was taken when the knife edges were viewed as dark objects on a white ground, and also when they were seen as light objects on a black ground. The difference of such readings will obviously give double the correction required. The results are contained in the following table.

Divisions of the microm. the ground being <i>white</i>	Divisions of the microm. the ground being <i>black</i> .	Difference.
32,0	44,0	12,00
19,5	30,0	10,50
17,5	28,0	10,50
16,5	27,7	11,20
12,5	25,0	12,50
12,5	22,0	9,50
12,0	23,0	11,00
10,0	21,0	11,00
9,7	18,0	8,30
5,5	19,0	13,50
5,7	16,5	10,80
5,0	16,5	11,50
Mean		11,03

From the above table it seems that 5,51 divisions (or .000236 of an inch) are to be subtracted from the distance

obtained when the knife edges are viewed as dark objects on a light ground; and on the contrary, the same quantity to be added when they are seen as light objects on a dark ground.

From the few experiments I have made, this quantity appears to be the same, whatever may be the relative illumination of the object and its ground, so long as the difference of character is preserved. On the cause of this extraordinary fact I can hazard no conjecture, and it remains an interesting subject for future investigation.

Of the expansion of the pendulum.

The composition of brass is so various, that probably no two specimens possess precisely the same rate of expansion. It became therefore necessary to determine the expansion of the pendulum by direct experiment, instead of adopting the conclusions of others, and for this purpose the following method was used. A trough of deal was made of a length sufficient to receive the bar intended for the pendulum, which was placed edgewise in the middle of the trough, being secured at one end by wedges on both sides. The bar was supported on small pieces of glass tube, serving as rollers to prevent friction, and the trough was of the same depth as the width of the bar.

Two transverse lines were drawn near the extremities of the edge of the bar, distant from each other 49.5 inches, and a third line was subsequently drawn one inch beyond. The microscopes were placed over the lines, and left, together with a thermometer, for twenty four hours previous to the experiment.

The temperature being then registered, and the micro-

scopes having been examined to see that the lines bisected the angles formed by the spider's threads, the trough was filled with hot water to the edge of the bar, and two thermometers were placed in it, one just beneath the surface of the water, and the other at the bottom of the trough. The bar rapidly expanded, and the line on it was followed by the micrometer till it became stationary. The bisection was then perfected, and the mean of the degrees shewn by the thermometers registered, together with the number of revolutions and parts made by the micrometer. The whole was now suffered to remain till the temperature had become several degrees lower, when the contraction of the bar, occasioned by such decrease of temperature was measured, and thus several successive observations were made, which are contained in the following table.

Distance between the lines on the bar, 49,5 inches.				
Highest Temp.	Lowest Temp.	Diff of Temp.	Divis. of the micrometer.	Expansion in parts of the length for each degree.
96	43	53	620	,000010116
93	43	50	580	,000010030
Distance between the lines on the bar, 50,5 inches.				
91	43	48	600	,000010616
89	84	5	70	,000011890
83	75	8	89	,000009448
75	61	14	149	,000009038
80	44	36	400	,000009436
80	60	20	215	,000009129
73	60	13	152	,000009930
Mean of the whole,				,000009959

The mean, ,000009959 may be taken as the expansion of the pendulum in parts of its length due to a change of temperature of one degree of the thermometer.

Of the method of deducing the length of the pendulum vibrating seconds.

The distance between the knife edges was taken when the standard scale and the pendulum were both of the same temperature; and as this temperature did not differ considerably from 62° , the difference in the rate of the expansion (if any) between the pendulum and the scale may be neglected as perfectly insensible, and 62° be considered as the temperature of measurement.

The number of vibrations made by the pendulum in 24 hours, having been determined at a different temperature, the length of the pendulum will be greater or less as the temperature of observation exceeds or falls short of 62° ; and by applying the expansion due to such difference of temperature, derived from the experiments contained in the preceding article, the distance of the knife edges, or length of the pendulum will be known for the temperature at which the number of vibrations was determined, whence the length of the pendulum vibrating seconds may be readily deduced, the lengths of pendulums being to each other inversely in the duplicate ratio of the number of their vibrations in 24 hours.

Of the correction for the buoyancy of the atmosphere.

The length of the pendulum thus found, differing from what it would have been had the vibrations been made in

vacuo, it is necessary to apply to it a correction for the buoyancy of the atmosphere.

For this correction, the weight of the pendulum, compared with that of air, at the time of observation, must be known.

The pendulum being composed of different kinds of brass, the specific gravity of each part was carefully determined, and from thence the specific gravity of the whole mass.

Part of the pendulum.	Weight in air.	Specific gravity.
	lb.	
3 weights (cast brass)	3,14	8,417
4 knee pieces (cast brass)	3,13	7,816
Bar (plate brass)	3,30	8,532

From the above data, the specific gravity of the pendulum is 8,469; or the weight of the pendulum compared with water is as 8,469 to 1.

It has been determined by Sir GEORGE SHUCKBURGH (Phil. Trans. for 1777) that water is 836 times heavier than air, when the thermometer is at 53° , and the barometer at 29,27 inches. But the specific gravity of air varies directly as the height of the barometer, and inversely as its expansion, which is known to be $\frac{1}{480}$ th part of its bulk for each degree of FAHRENHEIT; consequently, for any other state of the barometer and thermometer, the number 836, will vary *inversely* as the height of the barometer, and *directly* $\frac{1}{480}$ th part for each degree of the thermometer above 53° .

Thus the specific gravity of water, compared with that of air, may be known for the temperature and altitude of the barometer at the time of observation; and multiplying this

by the specific gravity of the pendulum, the ratio of the weight of the pendulum compared with that of air will be obtained.

This ratio will express the diminution of the force of gravity arising from the buoyancy of the atmosphere; and in order that the number of vibrations may be the same in vacuo as in air, the length of the pendulum must be increased in the proportion of this ratio to 1, the lengths of pendulums vibrating in the same time, varying directly as the force of gravity.

Detail of the experiments.

In the first experiments which were made with the pendulum, it has been already observed that the knife edges rested on plates of hard steel, but as these at the conclusion were found to have suffered penetration in no slight degree, planes of agate were substituted for them, and the results having thus been rendered doubtful, were deemed inadmissible. It may not however be irrelevant to remark, that the distances of the knife edges obtained by the two methods which have been before described, did not differ quite one ten thousandth of an inch; and, that on re-measurement after the knife edges had been used a very considerable time, their distance was found to be increased by wear, four divisions only of the micrometer, or not quite two ten thousandths of an inch. The length of the seconds pendulum deduced from these first experiments, differed from the result of the observations about to be detailed, only two ten thousandths of an inch *in defect*. I nevertheless think it useless to insert these first experiments, as the near approximation of the result cannot but be deemed to have been in some degree accidental.

In repairing the knife edges after the termination of the first series of experiments, one of them was broken, and when it was replaced by another, the distance between them was increased about one hundredth of an inch, a circumstance which proved rather gratifying than otherwise, as it afforded a pendulum differing in length from the former one, and which yet gave nearly the same result.

June 9th, 1817, the knife edges being adjusted parallel to each other, and the scale and pendulum having remained together for several preceding days, the pieces A, a, and B, b, were applied to the knife edges in the manner described in the former part of this paper, and the following measurements were taken.

Distance from A to a, 329,06 divisions. B to b, 366,97				
Date.	Readings of the micrometer.			Divisions + 39.4 in.
	A to a.	B to b.	Scale.	
June 9th.	27,0	62,0	653,0	956,51
	21,0	52,0	642,7	954,21
	13,0	52,0	642,5	958,01
	12,0	48,0	638,5	956,51
	18,0	50,0	643,0	957,01
	18,0	50,0	642,0	956,01
The pieces changed.				
10th.	65,5	112,7	698,0	956,91
	64,0	112,5	696,0	955,76
	61,0	106,2	693,2	957,61
	64,5	108,0	693,5	955,26
	65,0	106,2	694,5	956,91
	67,2	107,0	696,0	956,91
Mean of the whole				956,47

Hence the distance between the knife edges is 39.4 inches + 956.47 divisions of the micrometer.

June 12th, the knife edges having been adjusted parallel to each other, the following measurements were taken, the knife edges being viewed as *dark* objects on a *white* ground.

Dark on a white ground.					
Date.	Readings of the micrometer.				Divisions + 39.4 in.
	Near side of the bar.	Further side of the bar.	Mean.	Scale.	
June 12	50.0	50.0	50.00	1006.5	956.50
	50.0	50.0	50.00	—	956.50
	50.0	50.0	50.00	—	956.50
	49.0	50.0	49.50	—	956.50
12	46.5	44.0	42.25	1001.0	955.75
	44.5	44.5	45.50	1001.0	955.50
	42.0	43.0	42.50	1003.0	960.50
	43.0	43.0	43.00	1003.0	960.00
13	37.5	38.0	37.75	994.0	956.25
	35.0	39.0	37.00	993.5	956.50
	38.0	35.0	36.50	1001.0	964.50
	38.0	38.0	38.00	—	963.00
14	25.5	27.5	26.50	987.0	960.50
	25.0	26.5	25.75	987.5	961.75
	24.0	25.2	24.60	—	962.90
	25.0	25.7	25.25	—	962.25
14	79.5	78.0	78.75	1042.0	963.25
	76.0	75.0	75.50	—	966.50
	72.0	71.5	71.75	1035.2	963.45
	74.0	73.5	73.75	—	961.45
Mean of the whole					960.00
Correction for Irradiation					— 5.51
(see page 59)					954.49

By the foregoing measurements, the distance between the knife edges appears to be 39,4 inches +954.49 divisions.

The pendulum was now placed on its support, and the following experiments made for equalising the number of vibrations.

Slider 18 divisions. Clock losing 0".33 on mean time.		Great weight above.						Barometer 29.7.	
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
June 19th.	66,8	m. s. 29.12	1,29	1,18	518	516	86066,40	s 2,28	86068,68
		37.50	1,07	1,00	520	518	86067,70	1,63	86069,33
	66,8	46.30	0,93					Mean Clock	86069,00 — 0,33
	66,8	mean							86068,67
	Great weight below.								
	66,6	5,59	1,25	1,19	513	511	86063,16	2,32	86065,48
		14,32	1,13	1,06	512	510	86062,50	1,84	86064,34
	66,7	23,4	1,01					Mean Clock Temp.	86064,91 — 0,33 — 0,04
	66,7	mean							86064,54

Here the vibrations were in excess; the slider was therefore placed at 29 divisions, and the second weight moved nearer to its knife edge.

Slider 29 divisions. Clock losing 0",33 on mean time.		Great weight above. Second weight moved.						Barometer 29.7.	
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
June 19th	67,0	m. s. 57.42	0 1,23	0 1,13	506	504	86058,49	5 2,08	86060,57
	66,9	6. 8	1,04	0,96	509	507	86060,50	1,51	86062,01
		14.37	0,88					Mean Clock	86061,29 — 0,33
	66,9	mean.							86060,96
Great weight below.									
	67,1	24.39	1,14	1,07	506	504	86058,49	1,87	86060,36
	67,1	33. 5	1,01	0,97	507	505	86059,16	1,51	86. 60,67
		41.32	0,93					Mean Clock Temp.	86060,51 — 0,33 + 0,09
	67,1	mean.							86060,27

The number of vibrations being still in excess, the second weight was moved again, the slider remaining as before.

Slider 29 divisions. Clock losing 0",26 on mean time.		Great weight above.						Barometer 29,76	
	Temp.	Time of co- incidence.	Arc of vibration	M in arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
June 20th.	68,7	m. s.	•	•				s.	
		1.17	1,22	1,13	503	501	86056,47	2,08	86058,55
		9.40	1,04	0,96	503	501	86056,47	1,51	86058,98
		18. 3	0,88	0,80	505	503	86057,82	1,05	86058,87
	68,7	26.28	0,73	0,68	504	502	86057,16	0,75	86057,91
		34.52	0,63						
								Mean Clock	86058,33 — 0, 6
	68,7	mean							86058,07
Great weight below.									
	68,4	22.4	1,19	1,13	504	502	86057,16	2,08	86059,24
		30.28	1,07	1,00	504	502	86057,16	1,63	86058,79
		38.52	0,94	0,90	504	502	86057,16	1,38	86058,54
		47.16	0,86	0,82	505	503	86057,82	1,10	86058,92
	68,5	55.41	0,78						
								Mean Clock Temp.	86058,87 — 0,26 — 0,09
	68,5	mean							86058,52

The number of vibrations being now in both positions sufficiently near each other, and in *defect*, the second weight was secured in its place.

Slider 23 divisions. Clock losing 0",20 on mean time		Great weight above.						Barometer 29,86.	
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
June 21st.	0	m. s.	0					s.	
	71,4	36.6	1,22	1,12	502	500	86055,78	2,05	86057,83
		44.28	1,03	0,95	503	501	86056,47	1,48	86057,95
		52.51	0,87	0,80	503	501	86056,47	1,04	86057,51
		1.14	0,73	0,67	506	504	86058,49	0,73	86059,22
	71,4	9.40	0,62						
								Mean Clock	86058,13 — 0,20
	71,4	mean							86057,93
	Great weight below.								
	71,5	20.24	1,19	1,13	502	500	86055,78	2,08	86057,86
		28.46	1,07	1,00	503	501	86056,47	1,63	86058,10
		37. 9	0,94	0,89	503	501	86056,47	1,29	86057,76
		45.32	0,85	0,81	503	501	86056,47	1,07	86057,54
	71,6	53.55	0,78						
								Mean Clock Temp.	86057,81 — 0,20 + 0,09
	71,6	mean							86057,70

Slider 23 divisions. Clock gaining 0",30 on mean time.		Great weight <i>above</i> .					Barometer 29,95.		
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
June 23rd.	73,0	m. s.	°	°				s	
		9. 8	1,22	1,12	500	498	86054,40	2,05	86056,45
		17.28	1,02	0,94	501	499	86055,09	1,44	86056,53
		25.49	0,87	0,80	501	499	86055,09	1,04	86056,13
		34.10	0,74	0,68	501	499	86055,09	0,75	86055,84
	73,1	42.31	0,63						
								Mean Clock	86056,24 + 0,30
73,1	mean							86056,54	
D. Great weight <i>below</i> .									
	72,4	27.33	1,21	1,15	501	499	86055,09	2,16	86057,25
		35.54	1,09	1,04	501	499	86055,09	1,76	86056,85
		44.15	0,99	0,94	501	499	86055,09	1,44	86056,53
		52.36	0,89	0,84	502	500	86055,78	1,15	86056,93
		0.58	0,79						
	72,8							Mean Clock Temp.	86056,89 + 0,30 — 0,22
72,6	mean.							86056,97	

The pendulum was now taken down to re-measure the distance between the knife edges, in order to ascertain whether or not they had suffered from use.

The pieces A, a, B and b, being applied as before, the following measurements were taken.

Distance from A to a, 329,06 divisions. B to b, 366,97.				
Date.	Readings of the micrometer.			Divisions. + 39,4 in.
	A to a.	B to b.	Scale.	
June 25th.	9,7	39,0	630,0	953,66
	7,0	37,3	630,0	955,86
	10,0	36,5	630,7	955,46
The pieces made to change places				
26th.	59,0	87,0	680,0	955,01
	59,0	84,0	680,3	956,81
	51,0	75,0	671,0	955,51
	43,0	67,7	664,5	957,16
	41,5	68,0	662,5	955,76
Mean of the whole				955,65

Hence the distance between the knife edges is 39,4 inches +955,65 divisions of the micrometer.

Having thus satisfied myself that no injury to the knife edges was to be apprehended from moderate use, the pendulum was again suspended, but now, to my surprise, I found the number of vibrations different from what they were before the re-measurement. This difference became still greater on the following day, and it at length occurred to me that the moisture of the atmosphere must have undergone some change, and that an alteration had been thus occasioned in the weight of the wooden extremities of the pendulum. On referring to the register of the hygrometer

kept by Mr. BROWNE, it was found that a considerable change had in fact suddenly taken place from moisture to dryness; and, so great was the derangement of the pendulum from this apparently trivial cause, that it became necessary to move the second weight. This was accordingly done, and the following experiments made for again bringing the number of vibrations to an equality.

Slider 29 divisions. Clock gaining 0 ^u ,18 on mean time.		Great weight <i>above</i> . Second weight moved.						Barometer 29,70.	
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 1st.	69,1	M. S. 10.31	1,21	1,11	501	499	86055,09	5, 2,01	86057,10
		18.52	1,01	0,92	502	500	86055,78	1,38	86057,16
	69,1	27.14	0,83					Mean Clock	86057,13 + 0,18
	69,1	mean.							86057,31
	Great weight <i>below</i> .								
	68,8	47.22	1,23	1,16	502	500	86055,78	2,20	86057,98
	68,9	55.44 4. 6	1,09 0,99	1,04	502	500	86055,78	1,76	86057,54
								Mean Clock Temp.	86057,76 + 0,18 — 0,09
	68,9	mean							86057,85

The second weight was now securely fixed.

Slider 21 divisions. Clock gaining 0",18 on mean time.		Great weight <i>above</i> .						Barometer 29.70.	
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
July 1st.	a	m. s.	°	°				°	
	69,3	28. 9	1,23	1,13	503	501	86056,47	2,08	86058,55
		36.32	1,03	0,96	504	502	86057,16	1,51	86058,67
	69,3	44.56	0,89	0,81	505	503	86057,82	1,07	86058,89
		53.21	0,74					Mean Clock	86058,70 + 0,18
	69,3	mean							86058,88
<div style="text-align: center;"> E. Great weight <i>below</i>. </div>									
	69,3	49. 6	1,21	1,15	503	501	86056,47	2,16	86058,63
		57.29	1,09	1,03	504	502	86057,16	1,73	86058,89
		5.53	0,98	0,93	504	502	86057,16	1,41	86058,57
	69,3	14.17	0,88	0,83	505	503	86057,82	1,13	86058,95
		22.42	0,78					Mean Clock	86058,76 + 0,18
	69,3	mean.							86058,94

Slider 18 divisions.

Clock gaining 0".18
on mean time.

Great weight above.

Barometer
29.70.

	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.	
July 2nd.	68,6	m. s.	°	°				s.		
		18.31	1,25	1,14	504	502	86057,16	2,12	86059,28	
		26.55	1,04	0,96	504	502	86057,16	1,50	86058,66	
		35.19	0,89	0,81	507	505	86059,16	1,07	86060,23	
		43.46	0,74	0,68	505	503	86057,82	0,75	86058,57	
	68,7	52.11	0,63					Mean Clock	86059,18 + 0,18	
		68,7	mean						86059,36	
	H.	Great weight <i>below</i> .								
		68,8	57.41	1,23	1,16	503	501	86056,47	2,20	86058,67
			6. 4	1,10	1,05	504	502	86057,16	1,80	86058,96
14.28			1,00	0,94	504	502	86057,16	1,44	86058,60	
23.52			0,88	0,85	506	504	86058,49	1,18	86059,67	
31.18			0,82					Mean Clock Temp.	86058,98 + 0,18 + 0,09	
69,1										
		68,9	mean						86059,25	

Slider 18 divisions. Clock gaining 0",18 on mean time.			Great weight above.					Barometer. 29,70.	
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 2nd.	69,4	m. s.	°	°				s.	
		17.43	1,24	1,13	504	502	86057,16	2,08	86059,24
		26. 7	1,03	0,95	504	502	86057,16	1,47	86058,63
		34.31	0,88	0,81	504	502	86057,16	1,07	86058,23
	69,3	42.55	0,74	0,69	507	505	86059,16	0,78	86059,94
		51.22	0,64						
								Mean Clock	86059,01 + 0,18
	69,3	mean							86059,19
I.	Great weight below.								
	69,3	38.22	1,20	1,14	502	500	86055,78	2,12	86057,90
		46.44	1,08	1,03	505	503	86057,82	1,73	86059,55
		55. 9	0,98	0,93	504	502	86057,16	1,42	86058,58
		3.33	0,88	0,84	505	503	86057,82	1,15	86058,97
	69,3	11.58	0,80						
								Mean Clock	86058,75 + 0,18
	69,3	mean							86058,93

Slider 18 divisions.
Clock gaining 0', 18
on mean time.

Great weight *above*.

Barometer
29.70.

	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 2nd.	69.3	m. s.	°	°				s	
		53.29	1.31	1.19	503	501	86056.47	2.31	86058.78
		1.52	1.08	1.00	504	502	86057.16	1.63	86058.79
		10.16	0.92	0.84	506	504	86058.49	1.15	86059.64
		18.42	0.76	0.71	505	503	86057.82	0.82	86058.64
	69.3	26. 7	0.66					Mean Clock	86058.96 + 0.18
	69.3	mean							86059.14
	K. Great weight below.								
	69.3	32.18	1.24	1.17	502	500	86055.78	2.23	86058.01
		40.40	1.10	1.05	504	502	86057.16	1.80	86058.96
49. 4		1.00	0.95	505	503	86057.82	1.47	86059.29	
57.29		0.90	0.85	504	502	86057.16	1.18	86058.34	
5.53		0.81						Mean Clock	86058.65 + 0.18
69.3	mean							86058.83	

Slider 19 divisions. Clock gaining 0",18 on mean time.		Great weight <i>above</i> .						Barometer 29,90.		
	Temp.	Time of co- incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.	
July 3rd.	68,3	m. s.	°	°				s		
		45. 3	1,23	1,13	504	502	86057,16	2,08	86059,24	
		53.27	1,03	0,95	504	502	86057,16	1,47	86058,63	
		1.51	0,87	0,80	505	503	86057,82	1,04	86058,86	
		10.16	0,74	0,68	506	504	86058,49	0,75	86059,24	
	68,4	18.42	0,63					Mean Clock	86058,99 + 0,18	
										86059,17
	68,4	mean								86059,17
	Great weight <i>below</i> .									
	M.	68,4	24.31	1,24	1,17	503	501	86056,47	2,23	86058,70
32.54			1,11	1,05	504	502	86057,16	1,80	86058,96	
41.18			0,99	0,94	504	502	86057,16	1,44	86058,60	
49.42			0,90	0,86	506	504	86058,49	1,20	86059,69	
58. 8			0,82					Mean Clock Temp.	86058,99 + 0,18 + 0,04	
68,5										
68,5		mean								86059,21

The results of such of the preceding experiments as are to be used for calculating the length of the seconds pendulum, are brought under one view in the following table :

Place of the slider	Expt.	Temp.	Barom.	No. of vibrations. Great wt. above.	Diff.	No. of vibrations. Great wt. below.	Vibs. in excess or defect.
23	A	68,7	29,76	86059,39	,03	86059,42	—
23	B	71,3	29,86	86057,70	,23	86057,93	—
23	C	71,4	29,86	86057,93	,23	86057,70	+
23	D	73,1	29,95	86056,54	,43	86056,97	—
Pendulum re-measured.							
21	E	69,3	29,70	86058,88	,06	86058,94	—
20	F	69,3	29,70	86058,89	,12	86059,01	—
20	G	68,5	29,70	86059,03	,19	86059,22	—
18	H	68,7	29,70	86059,36	,11	86059,25	+
18	I	69,3	29,70	86059,19	,16	86058,93	+
18	K	69,3	29,70	86059,14	,31	86058,83	+
19	L	68,1	29,90	86059,26	,04	86059,22	+
19	M	68,4	29,90	86059,17	,04	86059,21	—
			Mean	86058,71		86058,72	

No other explanation of this table appears to be necessary, than that the column entitled " Difference " expresses the difference between the number of vibrations in the two positions of the pendulum, and that the last column indicates by the sign + or — whether the number of vibrations exceeds or falls short of the truth; which inference is drawn from a comparison of the number of vibrations when the great weight is above, with the number in that position of the pendulum when the great weight is below. The mean of the vibrations in the column " Great weight above" not differing sensibly from that headed " Great weight below," is a proof that the number of vibrations in either position of the pendulum may be considered as equal, and consequently that the

one knife edge being the point of suspension, the other must necessarily be in the centre of oscillation.

Length of the pendulum vibrating seconds.

The distance between the knife edges was as follows :

	Inches.	Divisions.	Inches.
By the 1st measurement	39.4	+ 956.47	= 39.44094
By the 2nd, -	39.4	+ 954.49	= 39.44086
By the 3d, -	39.4	+ 955.65	= 39.44090

Mean 39.44090

Corr. for error in division of the scale (see

page 54)

-

-

- 0.00005

39.44085

Hence, 39.44085 inches may be taken as the distance between the knife edges at the temperature of 62 degrees.

Using the vibrations when the great weight was *below*, as being nearer to the truth than in the other position of the pendulum, we obtain the following results.

Expt.	Temp.	Barom.	Vibrations in 24 hours.	Length of the seconds pen. in air	Corr. for the atmosphere.	Length of the sec. pend. in vacuo.	Difference from the mean.
A	68.7	29.76	86059.42	39.13313	.00544	39.13857	+,00028
B	71.3	29.86	86059.93	39.13278	.00544	39.13822	—,00007
C	71.4	29.86	86057.70	39.13260	.00544	39.13804	—,00025
D	73.1	29.95	86056.97	39.13259	.00544	39.13803	—,00026
E	69.3	29.70	86058.94	39.13293	.00544	39.13837	+,00008
F	69.3	29.70	86059.01	39.13298	.00544	39.13842	+,00013
G	68.5	29.70	86059.22	39.13286	.00545	39.13831	+,00002
H	68.7	29.70	86059.25	39.13296	.00544	39.13840	+,00011
I	69.3	29.70	86058.93	39.13291	.00544	39.13834	+,00005
K	69.3	29.70	86058.83	39.13282	.00544	39.13825	—,00003
L	68.1	29.90	86058.22	39.13271	.00548	39.13819	—,00009
M	68.4	29.90	86059.21	39.13281	.00548	39.13829	—,00000
					Mean	39.13829	

The length of the pendulum thus obtained requires yet another correction to reduce it to what it would have been at the level of the sea. The elevation of the apartments of the Royal Society at Somerset House above low-water mark, is known to be 81 feet; and by several careful observations with an excellent mountain barometer by RAMSDEN, I found the room in Portland Place, in which the experiments were made, to be two feet below the Royal Society's apartments; and as the height of the pendulum above the floor was four feet, we obtain 83 feet for the elevation of the pendulum above the level of the sea. Now the force of gravity increasing inversely as the square of the distance from the earth's centre, the length of the pendulum must be increased in the same proportion, and taking the radius of the earth for the latitude of Portland Place to be 3954.583 miles, we have 39,1386 inches for the length of the pendulum vibrating seconds at the level of the sea.

It may be remarked that the greatest difference between the mean result and that of any one of the twelve sets of experiments contained in the preceding table, is only .00028 of an inch, or $\frac{1}{134959}$ of the whole length of the pendulum; and as seven of the twelve sets are within one ten thousandth of an inch of the mean result, it may be inferred that the above determination cannot be very distant from the truth.

The length here given, is that required to perform one vibration in $\frac{1}{86400}$ part of a mean *solar* day, this being the measure of time usually employed for the purpose; but I am at a loss to conjecture why this is preferred to the *sidereal* day, a measure of time which marks a complete revolution

of the earth, and is readily obtained, being the interval between the returns of any fixed star to the meridian.

I shall now proceed to notice the sources of error which may be supposed to have affected the results of the preceding experiments.

These may be classed under the following heads.

1. The measurement of the distance of the knife edges.
2. The number of vibrations in 24 hours.
3. The temperature, and
4. The form of the knife edges.

On the first, it is scarcely necessary to offer any remark. Since the mean results of three several sets of measurements are within one ten-thousandth of an inch of each other, and the different methods employed, preclude, it may be presumed, any accidental coincidence, we may with confidence infer that the error in the distance of the knife edges, cannot amount to one ten-thousandth of an inch.

Among the number of vibrations in 24 hours given in the various sets of experiments, there appear to be differences which amount in some instances to 1,6. These differences however do not influence the truth of the result, beyond a certain minute quantity, the extent and origin of which I shall proceed to explain.

In order to determine the vibrations in 24 hours, it is necessary to ascertain the number of vibrations and parts of a vibration made by the brass pendulum during a certain number of *complete* seconds; but the moment of observation being limited to that when the brass pendulum is at the lowest part of the arc, the process is of necessity reversed, and the brass pendulum is observed to make a certain number of *complete* vibrations,

during a certain number of seconds and parts of a second which constitute *the interval*. The disappearance of the disk can however be noted only to a single second, and the brass pendulum may arrive at the lowest part of the arc either precisely at this second, or at any portion of a second preceding it. An error might possibly arise from this circumstance amounting to nine tenths of a second, by which the interval deduced from observation would be less than the truth, and as an error of one second in the interval, occasions a difference of 0,63 in the number of vibrations in 24 hours, if 0,55 (the proportional part of 0,63) be divided by 4 (the number of intervals forming each set of experiments) we have 0,14 for the greatest error *in defect* in the number of vibrations in 24 hours which can arise from this cause.

On the contrary, if the *second* coincidence or return of the brass pendulum to the lowest point of the arc, should have taken place nine tenths of a second before the second at which the disappearance of the disk was noted, the error in the number of vibrations in 24 hours would amount to the same quantity, and would now be *in excess*.

If the first and third coincidences take place accurately at the time of the observed disappearance of the disk, and the observation of the second coincidence should differ nine tenths of a second from the truth, it is obvious that the number of vibrations in 24 hours deduced from each interval will be erroneous about 0,56 the one being *in excess*, the other *in defect*: The mean of both will be the truth, though the observed difference between the two amounts to so considerable a quantity as 1,2.

The last coincidence of each set, takes place when the arc

of vibration is much reduced. It is therefore not impossible that an error of one second may sometimes, though rarely, occur in determining the time of this coincidence. This would occasion an error of about 0,63 in the number of vibrations in 24 hours, which divided by 4 as before, would influence the mean result 0,15 of a vibration.

In estimating these errors, I have taken an extreme case, as it is probable they would in most instances be compensated by the succeeding intervals. Supposing them however to be combined, the greatest effect on the mean result of any one set of experiments might amount to about 0,3 of a vibration in 24 hours, and the difference between the number of vibrations in either position of the pendulum, might have been double this quantity, and yet when the great weight was below, not have differed from the truth more than 0,3 of a vibration.

It appears then, that if the experiments have been conducted with sufficient care, no greater difference should be found between the mean, and any one of the resulting lengths of the pendulum contained in the preceding table, than might have been occasioned by a difference of 0,3 of a vibration in 24 hours, and this is found to be about 0,0003 of an inch.

In fact, on referring to the table we perceive that the experiments A and D, which differ most from the mean, give, the one, 00029 of an inch *in excess*, and the other, 00026 *in defect*.

In considering the sources of error, it may not be unnecessary to remark that had the bar of the pendulum been made too thick, and the knife edges not been placed accurately at right angles to it, an error, though very minute, might have arisen from the effect of the obliquity in diminishing the dis-

tance of the centre of oscillation from the axis. This was sufficiently guarded against by having the bar so thin as to ensure its becoming perpendicular by its own weight, had the position of the knife edge been in a small degree erroneous; for though the form the bar would assume is strictly speaking a curve, it may without sensible error be considered as a straight line.

With regard to temperature, every precaution was taken to prevent error. The thermometer used was made by Mr. TROUGHTON for the late Sir GEORGE SHUCKBURGH. It is divided into half degrees, and the height of the mercury may be estimated to one tenth of a degree. It has been already observed in the preceding part of this paper, that the thermometer was approached only at the first and last coincidences.

The experiments themselves afford, it is presumed, a sufficient proof of the stability of the knife edges. Every care was taken to form them in the first instance as perfect as possible; and after four sets of experiments had been made, they were found on re-measurement to have suffered no perceptible alteration; and it is evident by the near agreement of the results, that they remained uninjured during the succeeding experiments: it is difficult therefore to conceive that any error can have arisen from this source.

I may here remark, that the method I have employed in determining the length of the pendulum, possesses other advantages besides that of superseding the errors arising from unequal density or figure; and one, not the least considerable is, that after a very few vibrations, the true length of the pendulum is bounded by certain known limits. Thus in the two first sets of experiments, after the re-measurement of the

distance between the knife edges, we may remark that when the slider was at 29 divisions, the number of vibrations (the great weight being below) was, 86057,85 and in *defect*; and when the slider was removed to 19 divisions, the number of vibrations was 86059,41 and in *excess*. The true number of vibrations then is evidently between the two, and the utmost extent of error in using either of these numbers must fall short of 1,73 their difference when reduced to the same temperature. But if the mean be employed in the computation, the length of the pendulum will be found to differ only about four ten-thousandths of an inch from the mean result given in the foregoing table.

It may not be unnecessary to add that every experiment made has been retained; nor do I consider any one as less entitled to credit than the rest, excepting that marked A, in the table; and that, only because the rate of the clock was not observed on the day of the experiment, but was taken to be the same as the rate of the following day.

The length then of the pendulum vibrating seconds in vacuo at the level of the sea, measured at the temperature of 62° of FAHRENHEIT, appears to be

		inches.
By Sir G. SHUCKBURGH's standard	-	39,13860
By General ROY's scale	-	39,13717
By BIRD's Parliamentary standard	-	39,13842

the latitude of the place of observation being
51° 31' 8",4 north.*

* The latitude was deduced from the data contained in the trigonometrical survey; Mr. BROWNE's house bearing from Portland Chapel 74°. 38'. 50". west from the north, the distance being 283 feet. This differs only 0",1 from the latitude determined by Mr. BROWNE from a great number of observations.

An objection might be urged against the use of the knife edge, on the ground that being an elastic substance it may possibly suffer temporary compression, and thus perhaps introduce a source of error. In order to meet any doubt that might arise on this important part of the subject, it is my intention to commence a series of experiments with a pendulum of the same construction as that which has been described, but vibrating on cylinders instead of knife edges, and I trust soon to have the honor of laying the result before the Royal Society.

London, July, 1817.

APPENDIX.

Since the preceding Paper was written, a very curious and important theorem has been discovered by M. LAPLACE, of which Dr. YOUNG has favoured me with a concise demonstration, together with some other investigations which I shall subjoin in his own words.

MY DEAR SIR,

I cannot forbear to congratulate you on the discovery of the singular property of your pendulum, which has lately been demonstrated by M. LAPLACE, since it appears to remove the only doubt, that could reasonably be entertained, of the extreme accuracy of the results of your experiments. The correction for the curvature of the rolling surfaces, in the case of a simple pendulum, is very easily obtained from the geometrical determination of the curve described, although M. LAPLACE's train of reasoning, from mechanical principles, is somewhat too elaborate to be readily followed through all the symbols in which it is enveloped: and the same geometrical considerations appear, at first sight, to be equally applicable to the case of compound pendulums in general, since the motions of all their effective parts are concentric with those of a simple one similarly suspended. But upon further reflection, it becomes evident that these motions, though concentric, are related to each other in proportions somewhat different from those of a similar pendulum vibrating on a single point, and it is therefore necessary to deter-

mine the modification of the motion produced by this difference of connexion. The investigation may however be conducted in a method much more simple and intelligible to ordinary capacities, than that which has been adopted by the celebrated mathematician to whom we are indebted for the theorem; and I am tempted to send you an "*aperçu*" of the reasoning by which I have satisfied myself respecting it.

It follows immediately from the general theorem for finding the curvature of trochoids of all kinds, (Lectures on Nat. Phil. II. p. 559) that the radius of curvature of the path of any point, in the rod of a pendulum supported by a cylindrical axis, will initially be a third proportional to the distances of the point from the centre of the cylinder, and from the surface on which it rolls: so that when the cylinder is small, and the pendulum simple, the centre of curvature of its path may be considered as situated at the distance of the radius r below the point of contact: and this is obviously the only correction required for such a pendulum as that of BORDA. But when the weight is divided, or of considerable magnitude, it becomes necessary to calculate the effect of the different curvatures of the paths of its different parts, and to compare these paths with that of a pendulum A of any given length x . Supposing, for the sake of simplicity, the weight of each horizontal section to be concentrated in the vertical line, and calling the distance of any particle P below the surface of the cylinder x , the radius of curvature of its path will be a third proportional to $x+r$ and x , that is, $\frac{x^2}{x+r}$; and the inclination of the curve at a given distance from the vertical line being always directly as the curvature, or inversely as its radius, the force derived from the weight of P will be

to the force at an equal distance in the path of A, as a to $\frac{xx}{x+r}$, or as $\frac{a(x+r)}{xx}$ to 1. Now the point of the rolling pendulum confined to the vertical line is not the centre of curvature, but initially the surface of the cylinder: so that this must be considered as the point of intersection with the vertical line, and as the fulcrum of the lever; consequently the distance of P from the vertical line will be, to that of the pendulum A, as x to a , and its immediate force will be $\frac{a(x+r)}{xx} \cdot \frac{x}{a} \cdot P = \frac{x+r}{x} P$; but this force, acting only at the end of a lever x , will have its effect at A again reduced in the ratio of x to a , and will then become $\frac{x+r}{a} P$: and if we express the sum of all the similar forces belonging to the body by the character Σ , whether found by a fluxional calculation or otherwise, we have the whole force, at A, $\Sigma \frac{x+r}{a} P$. The reduced or rotatory inertia of the body, sometimes very improperly called the "momentum" of inertia, will also be expressed by $\Sigma \frac{xx}{aa} P$, being reduced in the ratio of the squares of the distances from the fulcrum; consequently the accelerative force

will be to that of the pendulum A as $\frac{\Sigma \frac{xx}{aa} P}{\Sigma \frac{x+r}{a} P}$ to 1, or as

$\frac{\Sigma xx P}{a \Sigma (x+r) P}$ to 1; since it is indifferent whether the integral or the differential be divided by the constant quantity a : and in order to express the length of the equivalent pendulum, we must suppose a to be as much lengthened as the force is weakened, so that we have for this length $\frac{\Sigma xx P}{\Sigma (x+r) P}$. It is obvious that the denominator of this fraction is the same that would express the force of the body with regard to the centre of the cylinder as a fixed point; and it might indeed

have been inferred at once, from the principle of virtual velocities, that the force must be the same in either case, however irregular the form of the body may be : but it is somewhat more satisfactory to follow the mechanical steps by which the operation of the law takes place. If we make $r=0$, we have $\frac{\sum xP}{\sum P} = l$, for the length of the equivalent pendulum when the surface of the cylinder is supposed to be the centre of suspension ; and it follows from the well known properties of the centre of gravity, that $\sum xP$ the sum of the product of all the particles into their distances, is equal to Qd , the product of the whole weight Q into the distance of the centre of gravity from the point of suspension ; and $\sum x^2P = \sum xP l = dQl$, so that the equivalent length for the rolling pendulum becomes $\frac{d l Q}{\sum (x+r)P} = \frac{d l Q}{\sum xP + \sum rP} = \frac{d l Q}{dQ + rQ} = \frac{l}{1 + \frac{r}{d}} = l(1 - \frac{r}{d})$,

r being supposed very small ; which, for a simple pendulum, when $d=l$, becomes $l-r$, as it ought to do. We must however find the displacement of the centre of suspension which is capable of producing an equal alteration in the length of the equivalent pendulum ; and for this purpose we must have recourse to the theorem of HUYGENS, which may be easily deduced from the expression $\frac{\sum xP}{dQ}$: for calling $x=d$, the distance of any particle of the body from its centre of gravity, y , we have $x^2 = (d+y)^2 = d^2 + 2dy + y^2$, and $\sum x^2P = \sum d^2P + 2d \sum yP + \sum y^2P = d^2Q + 0 + \sum y^2P$, the integral of $\sum yP$, the product of the distance of each particle into its distance from the common centre of gravity always vanishing : consequently $l = \frac{\sum y^2P + d^2Q}{dQ} = \frac{\sum y^2P}{dQ} + d$, and $l - d = \frac{\sum y^2P}{dQ}$; which is HUYGENS's theorem : the constant quantity $\frac{\sum y^2P}{Q}$ being equal

to $dl - d^2$. If now we suppose d to be increased by the small quantity s , the reciprocal, instead of $l - d$, will become $\frac{dl - dd}{d + s} = \frac{l - d}{1 + \frac{s}{d}} = (l - d) \left(1 - \frac{s}{d}\right) = l - d - l\frac{s}{d} + s$, to which

adding $d + s$, we have $l - l\frac{s}{d} + 2s$, the increase of the length being $\frac{2d - l}{d}s$; and making this equal to $-\frac{l}{d}r$, we have $s = \frac{-lr}{2d - l}$; and when the pendulum is inverted, substituting $l - d$ for d , the expression becomes $\frac{-lr}{2l - 2d - l} = \frac{lr}{2d - l}$, which, added to the former negative value of the same quantity, must always destroy it: so that the length of the equivalent pendulum will be truly measured by the simple distance of the surfaces of the cylinders, as M. LAPLACE has demonstrated.

There is however another correction, of which it becomes necessary to determine the value, when a very sharp edge is used for the axis of motion, as in the pendulum which you have employed: since it appears very possible, that in this case the temporary compression of the edge may produce a sensible elongation of the pendulum. But it will be found, by calculating the magnitude of this change, that when the edge is not extremely short, and when its bearing is perfectly equable, this correction may be safely neglected.

Supposing a to be the distance from the edge, in the plane bisecting its angle, at which the thickness is such, that the weight of the modulus of elasticity corresponding to the section shall become equal to the weight of the pendulum, the elasticity at any other distance x from the edge will be measured by x , while the weight is represented by a ; so that the elementary increment x' will be reduced by the pressure of the weight to $\frac{x}{a + x}x'$, and the element of the compression

will be $\frac{a}{a+x} x'$, and its fluxion $\frac{a}{a+x} dx$, of which the fluent is $a \text{ HL } \frac{a+x}{a}$. Now the height of the modulus of elasticity of steel is ten million feet, (Lect. Nat. Phil. II. p. 509) and the weight of a bar, an inch square, and of this height, would be about 30 millions of pounds; so that if the weight be 10 pounds, and the line of bearing an inch long, the thickness at the distance a must be one three millionth of an inch; and supposing the angle a right one, a must be $\frac{1}{4244000}$; and making $x=1$, we have the whole compression of the edge within the depth of an inch $\frac{1}{4244000} \text{ HL } 4244001$; and this logarithm being 15.26, the correction becomes equal to the 360 thousandth of an inch. If the bearing were one tenth of an inch only, the compression for both the opposite edges would become $\frac{1}{180000}$, supposing that they retained their elasticity, and underwent no permanent alteration of form. In fact, however, the edge must be considered as a portion of a minute cylinder, which will be still less compressible than an angle contained by planes; and the happy property, demonstrated by M. LAPLACE, will prevent any sensible inaccuracy from this cause, however blunt the edges may be, supposing that the steel is of uniform hardness in both.

Believe me, my dear Sir, very sincerely yours,

THOMAS YOUNG.

Wellbeck Street, 5th Jan. 1818.

P. S. It is easy to show that the determination of the length of the pendulum, by means of a weight sliding on a rod or bar, which is the method that I have proposed as the most convenient for obtaining a correct standard, is equally independent of the magnitude of the cylinder employed. The

reduced inertia $\Sigma x^2 P$ here consists of two portions: for the rod we may take the equivalent expression dlQ , which we may call axy , a being the weight of the bar (Q), x the distance (d) of the centre of gravity, and y the equivalent length (l): for the ball we must employ the formula $\Sigma x^2 P = \Sigma y^2 P + d^2 Q$, and call $\Sigma y^2 P$, u , and $d^2 Q$, bzx^2 , b being the weight of the ball, and z the distance of its centre of gravity from the point of suspension: and in the same manner the force $\Sigma(x+r)P = (d+r)Q$ must be composed of the two portions $a(x+r)$ and $b(z+r)$, so that the equivalent length becomes

$$\frac{\frac{axy + u + bzx}{a(x+r) + b(z+r)}}{z + \frac{ax + ar + br}{b}} = \frac{z^2 + \frac{axy + u}{b}}{z + \frac{ax + ar + br}{b}}; \text{ which we may call } \frac{zx + v}{z + w} = t.$$

The experiment being then performed in four different positions of the weight, at the distances d' , d'' , and d''' , so that the second value of z may be $z - d' = z'$, the third $z - d'' = z''$, and the fourth $z - d''' = z'''$, we must observe the times of vibration, and deduce from them the comparative lengths of the equivalent pendulum, t , $n't$, $n''t$, and $n'''t$: and hence the value of z , of v , and of t may be obtained, without determining w , and of course without employing the quantity r .

$$\text{First, } \frac{z^2 + v}{z + w} = t, \frac{z'^2 + v}{z' + w} = n't, \frac{z''^2 + v}{z'' + w} = n''t, \frac{z'''^2 + v}{z''' + w} = n'''t.$$

$$\text{II, } z + w = \frac{z^2 + v}{t}, z' + w = \frac{z'^2 + v}{n't}, z'' + w = \frac{z''^2 + v}{n''t}, \\ z''' + w = \frac{z'''^2 + v}{n'''t}.$$

$$\text{III, } z - z' = d'; z - z'' = d''; z - z''' = d'''.$$

$$\text{IV, } d' = \frac{z^2 + v}{t} - \frac{z'^2 + v}{n't}; d'' = \frac{z^2 + v}{t} - \frac{z''^2 + v}{n''t}; d''' = \frac{z^2 + v}{t} - \frac{z'''^2 + v}{n'''t}.$$

$$V, t = \frac{z^2 + v}{d'} - \frac{z'^2 + v}{n'd'} = \frac{z^2 + v}{d''} - \frac{z''^2 + v}{n''d''} = \frac{z^2 + v}{d'''} - \frac{z'''^2 + v}{n'''d'''}.$$

VI, by comparing the first of these equations successively with the second and third, and bringing the terms containing v to the same side, we obtain

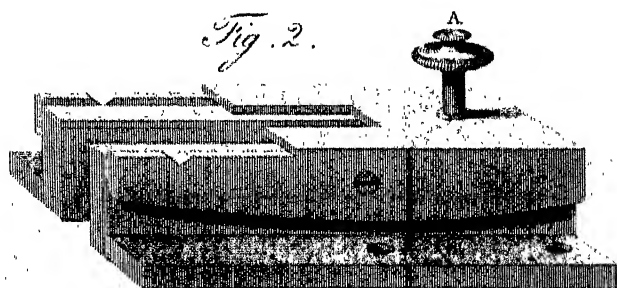
$$v = \left(\frac{z^2}{d'} - \frac{z'^2}{n'd'} - \frac{z^2}{d''} + \frac{z''^2}{n''d''} \right) : \left(\frac{1}{d'} - \frac{1}{n'd'} - \frac{1}{d''} + \frac{1}{n''d''} \right) = \left(\frac{z^2}{d'} - \frac{z'^2}{n'd'} - \frac{z^2}{d'''} + \frac{z'''^2}{n'''d'''} \right) : \left(\frac{1}{d'} - \frac{1}{n'd'} - \frac{1}{d'''} + \frac{1}{n'''d'''} \right).$$

This equation contains only the squares of the values of z with known coefficients; and if we substitute $z = d'$, $z = d''$, and $z = d'''$ for z' , z'' , and z''' , respectively, we shall obtain an equation in the form $ez^2 + fz = g$, whence $z = \pm \sqrt{(g + \frac{1}{4}f^2) - \frac{1}{2}f}$.

T. Y.



Fig. 2.



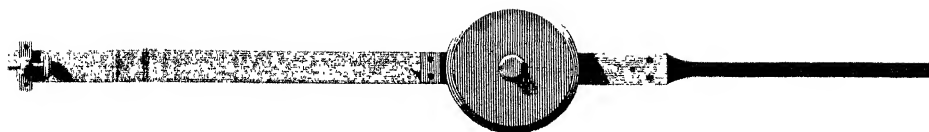
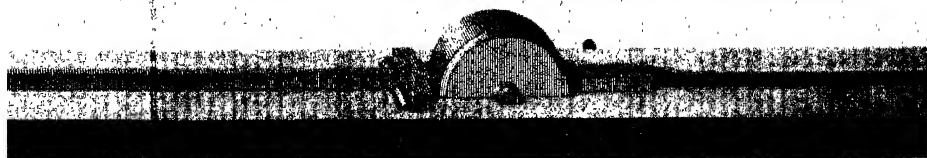
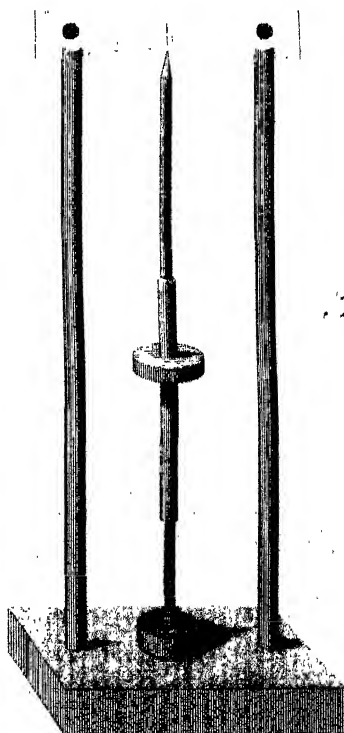
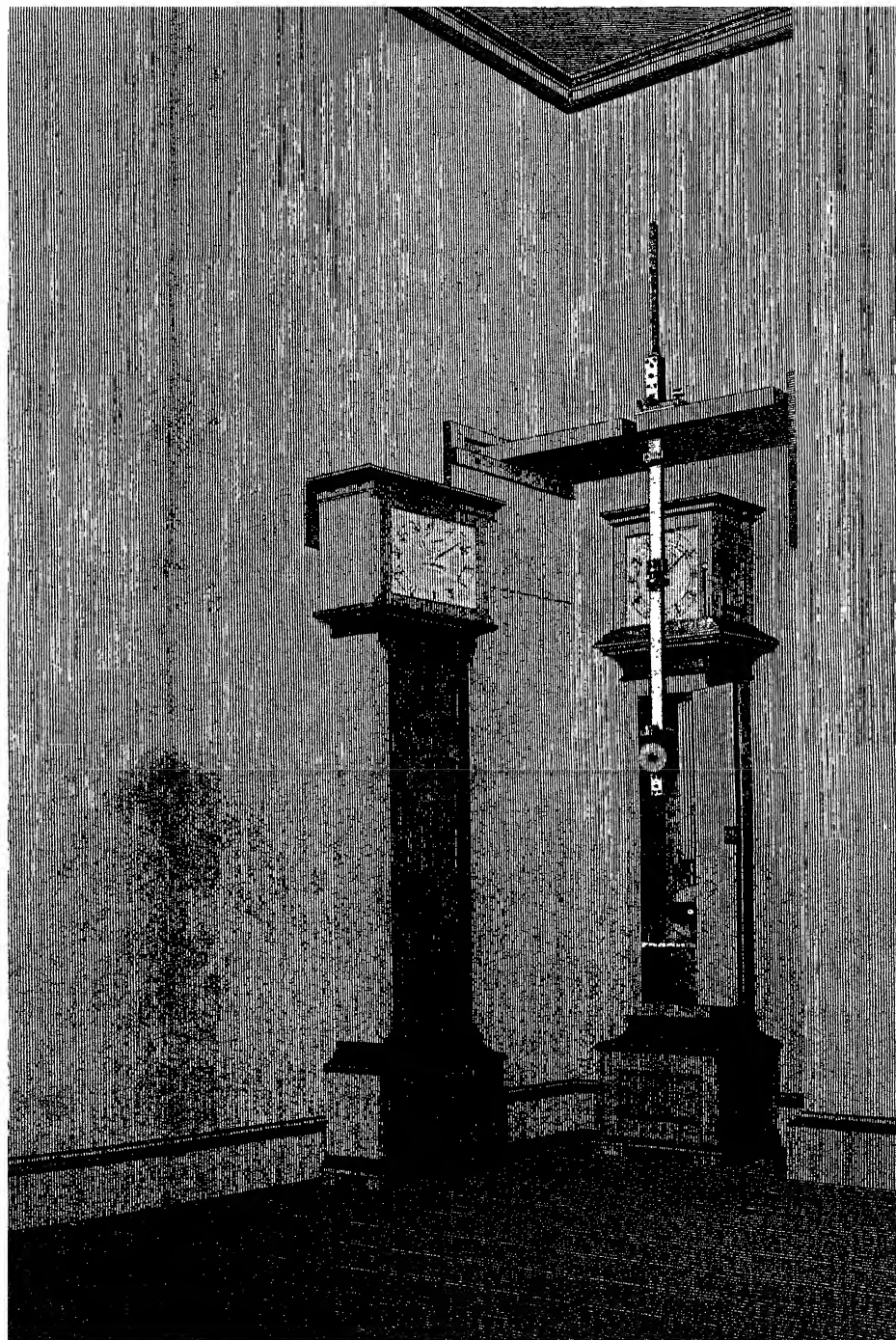


Fig. 3.





V. *On the length of the French Mètre estimated in parts of the English standard.* By Captain Henry Kater, F. R. S.

Read February 5, 1818.

ONE of the objects of the committee of the Royal Society appointed for the purpose of determining the length of the seconds pendulum, being the comparison of the French mètre with the British standard measure, two mètres were procured from Paris for that purpose, the one made in the usual manner and called the *mètre à bouts*, and the other a bar of platina on which the length of the mètre is shown by two very fine lines; this is named the *mètre à traits*.

The width of the *mètre à bouts* is one inch, and its thickness 0,3 of an inch. On one side the word "METRE" is engraved, and on the other "FORTIN à PARIS." The terminating planes are supposed to be perfectly parallel, and the distance between them is the length of the mètre.

The *mètre à traits* is the same width as the *mètre à bouts*, but only a quarter of an inch thick. The lines expressing the length of the mètre are so fine that one of them is scarcely perceptible even with the assistance of a microscope, unless the light be very favourable. The situation of the lines may however be discovered by two strong black dots made with a graver at the extremities of each, and a fine line crosses them at right angles to indicate the parts from which the measurements are to be taken.

This mètre previous to being brought from Paris, was

compared with a standard mètre by M. ARAGO, with all that care and ability which he is so well known to possess, and which so delicate an operation requires. The result was, that the distance between the lines was found to be less than a mètre by $\frac{17.52}{1000}$ of a millimètre or ,00069 of an inch.

The same micrometer microscopes were used in the comparisons which I am about to detail, as have been already described in my account of experiments on the length of the pendulum, in the Philosophical Transactions of the present year, and as the length of the mètre is nearly 39.4 inches, I was enabled to refer it to the same divisions of Sir GEORGE SHUCKBURGH'S scale as I had employed in the measurement of the pendulum.

I commenced with the *mètre à traits*. It was placed in contact with the standard scale, their surfaces being in the same plane. An excellent thermometer was laid upon the scale, and a piece of thick leather was placed upon its bulb in order to prevent its being affected by heat from the person of the observer.

The whole was suffered to remain in this state for two or three days, after which the following observations were made at various times, the microscopes being brought alternately over the mètre and the scale. The value of each division of the micrometer is $\frac{1}{23363}$ of an inch.*

* For the manner in which this value was obtained, see page 51 of the preceding paper.

Comparison of the *mètre à traits*.

Temperature.	Reading of the microm at 39,4 of the scale.	Reading of the microm at the mètre	Divisions to be deducted from 39,4 inches.	Distance in inches between the lines designating the mètre.	Correction for temperature in decimals of an inch.	Distance in inches between the lines designating the mètre, the mètre being at 32°, and the scale at 62°.
60,0	85,0	644,5	559,5	39,37606	,00604	39,37002
60,7	75,5	639,0	563,5	39,37588	,00589	39,36999
61,7	69,2	634,0	564,8	39,37583	,00568	39,37015
62,0	65,0	630,5	565,5	39,37580	,00562	39,37018
62,4	61,0	629,0	568,0	39,37569	,00554	39,37015
62,3	58,7	629,5	570,8	39,37557	,00556	39,37001
62,2	58,0	628,0	570,0	39,37560	,00558	39,37002
62,2	59,0	625,0	566,0	39,37577	,00558	39,37019
62,1	59,0	625,5	566,5	39,37575	,00560	39,37015
58,8	90,0	638,0	548,0	39,37654	,00629	39,37025
59,0	83,0	637,0	554,0	39,37629	,00625	39,37004
59,0	82,0	636,0	554,0	39,37629	,00625	39,37004
59,2	83,2	632,0	548,8	39,37651	,00621	39,37030
59,1	81,0	632,0	551,0	39,37642	,00623	39,37019
Mean						39,37012
The distance between the lines designating the mètre was found by M. ARAGO to be too little by a quantity = ,00069 of an inch which add						+ ,00069
The distance from zero to 39,4 of Sir G. SHUCKBURN'S scale is too short compared with the mean of its divisions by ,00005 of an inch which subtract*						— ,00005
Hence the length of the mètre in inches of Sir G. SHUCKBURN'S scale is						39,37076

The comparison of the *mètre à bouts*, presented considerable difficulties, which I conceive it would be of little use to detail, as the necessity of comparisons of this kind is of very rare occurrence; I shall therefore proceed to describe the method which was at last found successful.

* See page 54.

Four rectangular pieces of brass were prepared precisely similar to those described in the account of experiments on the pendulum in the Philosophical Transactions before referred to. These were marked C, *c*, D and *d*. The perfectly plane rectangular edges of the pieces C and *c*, being placed in contact, and kept thus by means of a spring, the distance of the fine lines drawn on their surfaces, parallel and very near to the rectangular edges, was found to be 500,5 divisions of the micrometer, and the pieces D and *d* being placed in like manner in contact, the distance of the lines on their surfaces estimated in the same divisions was 456,7.

The *mètre à bouts* being placed by the side of the brass scale and in contact with it, the pieces D and *d* were applied to its extremities, the surfaces of the brass pieces being a little below the surface of the *mètre* in order to preclude any error which might have arisen from the edges of the *mètre* projecting beyond its terminating planes. Each of the brass pieces was supported in this position upon a piece of lead of a sufficient thickness, and kept in close contact with the end of the *mètre* by means of a slight spring bearing against a pin driven perpendicularly into the lead.

In order to ensure a perfect contact between each brass piece and the terminating plane of the *mètre*, a flat ruler of brass was laid upon the surface of the *mètre* so as to project beyond its extremity, and the end of the lead was elevated or depressed so that the line of light seen between the piece of brass and the ruler, the eye being level with the surface of the brass, appeared to be equal in every part, when it was inferred that the surfaces of the *mètre* and of the piece of

brass were parallel, and consequently that their rectangular ends were perfectly in contact.

The distance between the lines on *D* and *d*, was now taken by the microscopes, and transferred to the scale in the manner before described; and when a sufficient number of comparisons had thus been made, the pieces *D* and *d*, were exchanged for those marked *C* and *c*, and the observations repeated with every precaution to ensure an accurate result, especially with respect to temperature.

The under surface of the mètre was then placed uppermost, and the apparatus being arranged as before, the same process was pursued as that which has just been described. The results are contained in the following tables.

Comparison of the *mètre à bouts*.

[illegible]

The pieces C and *c*, applied. Distance from C to *c*, 500,5 divisions. The word FORTIN above.

Temperature.	Reading of the microm. at 39,4 of the scale.	Reading of the microm. at the brass pieces.	Difference.	Divisions to be deducted from 39,4 inches.	Length of the Mètre.	Corr. for temperature in decimals of an inch.	Length of the Mètre, the mètre being at 32°, and the scale at 62°.
56,8	15,0	37,5	22,5	523,0	39,37762	,00671	39,37091
56,7	15,7	40,0	24,3	524,8	39,37754	,00673	39,37081
56,8	14,7	40,5	25,8	526,3	39,37747	,00671	39,37076
56,8	15,5	40,0	24,5	525,0	39,37753	,00671	39,37082
56,8	15,5	40,0	24,5	525,0	39,37753	,00671	39,37082
Mean							39,37082

The pieces D and *d*, applied. Distance from D to *d*, 456,7 divisions. The word FORTIN above.

55,0	35,3	93,0	57,7	514,4	39,37798	,00709	39,37089
55,2	36,5	95,0	58,5	515,2	39,37795	,00705	39,37090
55,2	36,0	93,0	57,0	513,7	39,37801	,00705	39,37096
56,0	26,5	89,0	62,5	519,2	39,37778	,00688	39,37090
56,0	23,5	87,0	63,5	520,2	39,37773	,00688	39,37085
Mean							39,37090

Summary of the preceding comparisons.

The word "MÈTRE" above - { D and *d*, 39,37084 } 39,37087
 { C and *c*, 39,37089 }

"FORTIN" { C and *c*, 39,37082 } 39,37086
 { D and *d*, 39,37090 }

Mean, 39,37086
 Subtract for error in division of the scale - ,00005

Length of the *Mètre à bouts* in inches of Sir G. SHUCK-
 BURGH'S scale } 39,37081

The following is the manner in which the correction for temperature was obtained. The expansion of platina according to the experiments of BORDA and others, is ,00000476

parts of its length for one degree of FAHRENHEIT, and as this is the expansion used by the French in adjusting the length of their mètre, it must be employed on the present occasion. The mètre being taken at 32° , the expansion for the difference between this and the temperature of measurement, must be subtracted from the apparent length of the mètre. The English standard temperature is 62° , therefore if the temperature of measurement be under this, the expansion of the scale for such difference of temperature must be deducted from the length of the mètre before obtained. These two corrections are combined in the column entitled "correction for temperature." Sir G. SHUCKBURGH's standard scale is of cast brass, and as I could not conveniently determine its actual expansion with that degree of accuracy that would have satisfied me, I have taken for it, the mean result of two experiments made on plate brass, which gave me an expansion of ,0000101 parts of its length for one degree of FAHRENHEIT. The mean of most of the experiments made on the expansion of brass gives ,0000104, and had I employed this last number instead of my own, the difference in the length of the mètre would have been utterly inconsiderable.

Supposing then both mètres to be of equal authority, we have for the length of the *mètre à traits* 39,37076, and for that of the *mètre à bouts* 39,37081 inches; the mean of which, 39,37079, may be taken for the length of the mètre in inches of Sir G. SHUCKBURGH's standard scale when each is brought to its proper temperature.*

* The length of the mètre compared with BIRD's parliamentary standard is 39,37062 inches.

VI. *A few facts relative to the colouring matters of some vegetables.* By James Smithson, Esq. F. R. S.

Read December 18, 1817.

I BEGAN, a great many years ago, some researches on the colouring matters of vegetables. From the enquiry being to be prosecuted only at a particular season of the year, the great delicacy of the experiments, and the great care required in them, and consequently the trouble with which they were attended, very little was done. I have now no idea of pursuing the subject.

In destroying lately the memorandums of the experiments which had been made, a few scattered facts were met with which seemed deserving of being preserved. They are here offered, in hopes that they will induce some other person to give extension to an investigation interesting to chemistry and to the art of dying.

Turnsol.

M. FOURCROY has advanced, somewhere, that turnsol is essentially of a red colour; and that it is made blue by an addition of carbonate of soda to it; and he says that he has extracted this salt from the turnsol of the shops.

If turnsol contained carbonate of soda, its infusions should precipitate earths and metals from acids.

I did not find an infusion of turnsol in water to have the least effect on solutions of muriate of lime, nitrate of lead, muriate of platina, or oxalate of potash.

Its tinctures, or infusions, consequently, contain neither any alkali, nor any lime ; nor probably any acid, either loose or combined. This is unfavourable to the opinion of urine being employed in the preparation of turnsol.

I put a little sulphuric acid into a tincture of turnsol, then added chalk, and heated ; and the blue colour was restored. It appears, therefore, that the natural colour of turnsol is not red, but blue, since it is such when neither disengaged acid or alkali is present.

No addition of chalk brought the cold liquor back to a blue colour ; the carbonic acid absorbed by it, during the effervescence of the carbonate of lime, being sufficient to keep it red.

Some turnsol was put into distilled vinegar. An effervescence arose ; and after some time the acid was become neutralized. On examining the mixture with a glass, there were seen, at the bottom of the vessel, a multitude of grains like sand. It was found on trial that these grains were carbonate of lime ; probably of slightly calcined Carrara marble.

When turnsol is treated with water till this no longer acquires any colour whatever, the remaining insoluble matter is nearly as blue as at first.

Acids made this blue insoluble matter red, but did not extract any red tincture.

Carbonate of soda did not affect it.

If the vegetable part of this blue residuum is burned away, or it is washed off with water, a portion of smalt is obtained.

On exhaling, on a water bath, a tincture of turnsol, the colouring matter is left in a dry state.

This matter heated in a platina spoon over a candle, tumbled considerably, as much as starch does, became black and smoked, but did not readily inflame, nor did it burn away till the blowpipe was applied. It then burned pretty readily, leaving a large quantity of a white saline matter. This saline matter saturated by nitric acid afforded crystals of nitrate of potash, and some minute crystals like hydrous sulphate of lime.

Is this potash merely that portion of this matter which exists in all vegetable substances? or is the colouring matter of turnsol a compound, analogous to ulmin, of a vegetable principle and potash? Its low combustibility gives some sanction to this idea.

Of the colouring matter of the violet.

The violet is well known to be coloured by a blue matter which acids change to red; and alkalies and their carbonates first to green and then to yellow.

This same matter is the tinging principle of many other vegetables: of some, in its blue state; of others, made red by an acid.

If the petals of the red rose are triturated with a little water and carbonate of lime, a blue liquor is obtained. Alkalies, and soluble carbonates of alkalis, render this blue liquor green; and acids restore its red colour.

The colouring matter of the violet exists in the petals of red clover, the red tips of those of the common daisy of the fields, of the blue hyacinth, the holly hock, lavender, in the inner leaves of the artichoke, and in numerous other flowers. It likewise, made red by an acid, colours the skin of several

plumbs, and, I think, of the scarlet geranium, and of the pomegranate tree.

The red cabbage, and the rind of the long radish are also coloured by this principle. It is remarkable that these, on being merely bruised, become blue ; and give a blue infusion with water. It is probable that the reddening acid in these cases is the carbonic; and which, on the rupture of the vessels which enclose it, escapes into the atmosphere.

Of sugar-loaf paper.

This paper has been employed by BERGMAN as a chemical instrument. I am ignorant of what it is coloured with.

Sulphuric, muriatic, nitric, phosphoric, and oxalic acids make it red. Tartaric and citric acids, made rather yellow spots than red ones. Distilled vinegar, and acid of amber, had no effect on it.

Carbonate of soda and caustic potash did not alter the blue colour of this paper.

Water boiled on this paper acquired a vinous red colour ; carbonate of lime put into this red liquor, did not affect its colour : nor did carbonate of soda or caustic potash change it to blue or green.

Cold dilute sulphuric acid extracted a strong yellow tincture from this boiled paper : carbonate of lime put to this yellow tincture made it blue ; but on filtering, the liquor which passed was of a dirty greenish colour ; and sulphuric acid did not make it red : a blue matter was left on the filter, which was not made red by acetous acid ; but was so by sulphuric.

After this treatment the paper remained brown; seemingly such as it was before being dyed blue.

It should seem that there are at least two colouring matters in this paper; one red, which is extricable from it by water; the other blue, which requires the agency of an acid to extract it.

Its insolubility in water, and low degree of sensibility to acids, distinguish the blue matter from turnsol; to which its not being affected by alkalis otherwise much approximate it. Its easy solubility in dilute sulphuric acid, and being reddened by it and several other acids, show it not to be indigo.

Of the black mulberry.

The expressed juice of this fruit is of a fine red colour.

Caustic potash made it green, which gradually became yellow.

Carbonate of soda did not make it green, but only blue.

Carbonate of ammonia changed it to a vinous red, rather than to blue; and this redness increased on standing.

Caustic ammonia made it bluer than its carbonate; but, on standing, the mixture became of the same vinous red.

The mulberry juice mixed with carbonate of lime became purple. On filtering, a red liquor passed; and the carbonate of lime left on the filter was blue. An addition of whitening to the red filtered liquor did not alter its colour; nor did this second portion of whitening become blue. Heating did not affect the red colour of this liquor; so that it was not owing to carbonic acid, disengaged from the carbonate of lime. Caustic potash instantly made this red liquor a fine green, and gradually yellow.

Sulphuric acid rendered all the above mixtures florid red. It is remarkable that the mixtures with ammonia, and carbonate of ammonia, which were become quite vinous red by standing, were made a perfect blue by the sulphuric acid before they were reddened by it. It would hence seem that the red colour, caused by these alkalis, was owing to an excess of them; and that in a less quantity they would have produced a blue.

The filter, into which the mixture of mulberry juice and chalk had been thrown, was become tinged blue. Water did not remove this colour. Sulphuric acid made this paper florid red. Caustic potash did not alter its blue colour; but put on the places made red by sulphuric acid, it restored the blue colour, but did not produce green.

Future experiments must decide whether this blue matter is the same as that of turnsol; or as the blue matter which the experiments above have indicated in sugar-loaf paper.

The juices of many other fruits, as black cherries, red currants, the skin of the berries of the buckthorn, elder berries, privet berries, &c., seem to be made only blue by mild fixed alkalis, but green by caustic. Puzzling anomalies, however, occasionally present themselves, which seem to show a near relation between the several blue colouring matters of vegetables, and their easy transition into one another.

The corn poppy.

The petals of the common red poppy of the fields rubbed on paper stain it of a reddish purple colour.

Solution of carbonate of soda put to this stain occasioned but little change in it.

Caustic potash made it green.

Caustic ammonia seemed not to have more effect on it than carbonate of soda.

Some poppy petals being bruised in a mixture of water and marine acid, formed a florid red solution : a superabundance of chalk added to this red liquor, did not make it blue; but turned it to a dark red colour exactly like port wine.

Some poppy petals bruised in a weak solution of carbonate of soda, and the mixture filtered, the liquor which came through was not at all blue, but of a dark red colour like port wine. Caustic potash made this red liquor green, which finally became yellow.

Some dried poppy petals of the shops, gave a strong obscure vinous tincture to cold water. This red tincture heated with whitening, did not alter to blue, but preserved its red colour.

These very imperfect experiments may perhaps suggest the idea, that the colouring matter of this flower is the same as the red colouring matter of the mulberry.

Of sap green.

The inspissated juice of the ripe, or semi-ripe, berries of the buckthorn, constitute the pigment called sap green; by the French, *vert de vessie*. This species of green matter is entirely different from the common green matter of vegetables.

It is soluble in water.

Carbonate of soda and caustic potash changed the solution of sap green to yellow. Paper tinged by sap green is a sensible test of alkalis.

Sulphuric, nitric, and marine acid, made it red. Carbonate of lime added to a reddened solution, restored the green colour, which therefore appears to be the proper colour of the substance.

The green colour, which the last infusions of galls present, appears to be different, both from the usual green of vegetables, and from sap green.

Some animal greens.

A green puceron, or aphid, being crushed on white paper, emitted a green juice, which was immediately made yellow by carbonate of potash (wrongly called sub-carbonate.)

There are small gnats of a green colour: crushed on paper, they make a green stain, which is permanent. Neither muriatic acid nor carbonate of soda altered this green colour. It is consequently of a different nature from the foregoing.

VII. *Account of experiments made on the strength of materials.*
 By George Rennie, jun. Esq. In a Letter to Thomas
 Young, M. D. For. Sec. R. S.

Read February 12th, 1818.

DEAR SIR,

London, June 3, 1817.

IN presenting you the result of the following experiments, I trust I shall not be considered as deviating from my subject, in taking a cursory view of the labours of others. The knowledge of the properties of bodies which come more immediately under our observation, is so instrumental to the progress of science, that any approximation to it deserves our serious attention. The passage over a deep and rapid river, the construction of a great and noble edifice, or the combination of a more complicated piece of mechanism, are arts so peculiarly subservient to the application of these principles, that we cannot be said to proceed with safety and certainty, until we have assigned their just limits. The vague results, on which the more refined calculations of many of the most eminent writers are founded, have given rise to such a multiplicity of contradictory conclusions, that it is difficult to choose, or distinguish, the real from that which is merely specious. The connections are frequently so distant, that little reliance can be placed on them. The Royal Society appears to have instituted, at an early period, some experiments on this subject, but they have recorded little to aid us. EMERSON, in his *Mechanics*, has laid down a number of rules,

and approximations. Professor ROBISON in his excellent treatise in the *Encyclopædia Britannica*; BANKS on the power of machines; Dr. ANDERSON of Glasgow; Colonel BEAUFOY, &c. are those, amongst our countrymen, who have given the result of their experiments on wood, and iron. The subject, however, appears to have excited considerable attention on the continent. A theory was published in the year 1638, by GALILEO, on the resistance of solids, and subsequently, by many other philosophers. But however plausible these investigations appeared, they were more theoretical than practical, as will be seen in the sequel. It is only by deriving a theory from careful and well directed experiments, that practical results can be obtained. It would be useless to enumerate the labours of those philosophers, who in following, or varying from the steps of GALILEO, have merely tended to obscure a subject respecting which they had no data to proceed upon. It is sufficient to enumerate the names of those who, in conjunction with our own countrymen, have added their labours to the little knowledge we possess. The experiments of BUFFON, recorded in the *Annals of the Academy of Sciences* at Paris, in the years 1740 and 1741, were on a scale sufficiently large to justify every conclusion, had he not omitted to ascertain the direct and absolute strength of the timber employed. It however appeared from his experiments, that the strength of the ligneous fibre is nearly in proportion to the specific gravity. MUSCHENBROECK, whose accuracy (it is said) entitled him to confidence, made a number of experiments on wood and iron, which by being tried on various specimens of the same materials, afforded a mean result considerably higher than other previous authorities. Experiments have

also been made by MARIOTTE, VARIGNON, PERRONET, RAMUS, RONDELET, GAUTHÉY, NAVIER, AUBRY and TEXIER DE NORBECK, as also at the Ecole Polytechnique, under the direction of M. PRONY. With such authorities before us, it might be deemed presumption in me, to offer you a communication on a subject which had been previously treated of by so many able men. But whoever has had occasion to investigate the principles upon which any edifice is constructed, where the combination of its parts are more the result of uncertain rules than sound principle, will soon find how scanty is our knowledge on a subject so highly important. The desire of obtaining some approximation, which could only be accomplished by repeated trials on the substances themselves, induced me to undertake the following experiments; for which purpose I ordered an apparatus to be prepared, of which the two annexed plates [Plates VI. and VII.] are representations.

Description of the Apparatus.

A bar of the best English iron, about 10 feet long, was selected and formed into a lever (whose fulcrum is denoted by *f*). The hole was accurately bored, and the pin turned, which suffered it to move freely. The standard (*A*) was firmly secured by the nut (*c*) to a strong bed plate of cast iron, made firm to the ground. The lever was accurately divided in its lower edge, which was made straight in a line with the fulcrum. A point, or division (*D*), was selected, at which place was let in a piece was balanced by the balance was ready for operation. But in order to keep it as level as possible, a hole was drilled

through a projection on the bed plate, large enough to admit a stout bolt easily through it, which again was prevented from turning in the hole by means of a tongue (*t*) fitting into a corresponding groove in the hole. So that, in order to preserve the level, we had only to move the nut to elevate, or depress the bolt, according to the size of the specimen. But as an inequality of pressure would still arise from the nature of the apparatus, the body to be examined was placed between two pieces of steel, the pressure being communicated through the medium of two pieces of thick leather above, and below the steel pieces, by which means a more equal contact of surfaces was attained. The scale was hung on a loop of iron, touching the lever in an edge only. I at first used a rope for the balance weight, which indicated a friction of four pounds, but a chain diminished the friction one half. Every moveable centre was well oiled. Of the resistances opposed to the simple strains which may disturb the quiescent state of a body, the principal are the repulsive force, whereby it resists compression, and the force of cohesion, whereby it resists extension. On the former, with the exception of the experiments of GAUTHEY and RONDELET, on stones, and a few others, on soft substances, there is scarcely any thing on record. In the memoir of M. LAGRANGE, on the force of springs, published in the year 1760, the moment of elasticity is represented by a constant quantity, without indicating the relation of this value to the size of the spring: but, in the memoir of the year 1770, on the forms of columns, where he considers a body whose dimensions and thickness are variable, he makes the moment of elasticity proportional to

the fourth power of the radius, in observing the relations of theory and practice to accord with each other. This was admitted by EULER in his memoir of 1780, in his elaborate investigation of the forms of columns. Mr. COULOMB had however shown before that time, how inapplicable all these calculations were to columns under common circumstances; and you, Sir, have repeated the observation in your lectures on natural philosophy. The results of experiments have also been equally discordant; since it is deduced from those of REYNOLDS, that the power required to crush a cubic quarter of an inch of cast iron, is 44800lbs. avoirdupoise, or 200 tons; whereas by the average of thirteen experiments made by me on cubes of the same size, the amount never exceeded 10392.53lbs, not quite five tons.* This may be seen by referring to the tables. There were four kinds of iron used, viz. 1st. iron taken from the centre of a large block, whose crystals were similar in appearance and magnitude to those evinced in the fracture of what is usually termed gun metal. 2ndly. Iron taken from a small casting, close grained, and of a dull grey colour. 3rdly. Iron cast horizontally in bars of $\frac{3}{8}$ th inches square, 8 inches long. 4thly. Iron cast vertically, same size as last. These castings were reduced equally on every side to $\frac{1}{4}$ of an inch square: thus removing the hard external coat usually surrounding metal castings. They were all subjected to a gauge. The bars were then presumed to be

* It is probable that Mr. REYNOLDS made his experiments on metal cast at the furnace of Maidley Wood, which is of a very strong and superior quality; but this circumstance can have been but of little importance compared to the great disproportion of the results.

tolerably uniform. The weights used were of the best kind that could be procured, and as the experiment advanced, smaller weights were used.

Experiments on cast iron in cubes of $\frac{1}{8}$ of an inch, &c.

Iron taken from the block whose specific gravity was 7,033.

Averages.					lbs. avoirdupoise.
1439.66	$\frac{1}{8} \times \frac{1}{8}$	-	-	-	1454
	$\frac{1}{8} \times \frac{1}{8}$	-	-	-	1416
	$\frac{1}{8} \times \frac{1}{8}$	-	-	-	1449

On specimens of different lengths. Specific gravity of iron 6,977.

2116	$\frac{1}{8} \times \frac{2}{8}$	-	-	-	1922
	$\frac{1}{8} \times \frac{2}{8}$	-	-	-	2310
1758.5	$\frac{1}{8} \times \frac{3}{8}$	slipped with 1863lbs. filed flat, and crushed with			2363
	$\frac{1}{8} \times \frac{4}{8}$	ditto,	1,495, ditto	-	2005
	$\frac{1}{8} \times \frac{5}{8}$	ditto,	-	-	1407
	$\frac{1}{8} \times \frac{6}{8}$	ditto,	-	-	1743
	$\frac{1}{8} \times \frac{7}{8}$	ditto,	-	-	1594
	$\frac{1}{8} \times \frac{8}{8}$	ditto,	-	-	1439

April 23d, 1817. Experiments on cubes of $\frac{1}{4}$ of an inch taken from the block.

9773.5	$\frac{1}{4} \times \frac{1}{4}$	-	-	-	10561
	$\frac{1}{4} \times \frac{1}{4}$	-	-	-	9596
	$\frac{1}{4} \times \frac{1}{4}$	-	-	-	9917
	$\frac{1}{4} \times \frac{1}{4}$	-	-	-	9020

Castings, Horizontal. Specific gravity 7.113.

				lbs. avoirdupoise.
Averages.	10114	$\frac{1}{4} \times \frac{1}{4}$	-	10432
		$\frac{1}{4} \times \frac{1}{4}$	-	10720
		$\frac{1}{4} \times \frac{1}{4}$	-	10605
		$\frac{1}{4} \times \frac{1}{4}$	-	8699
		$\frac{1}{4} \times \frac{1}{4}$	-	

Vertical castings. Specific gravity 7.074.

1113675	$\frac{1}{4} \times \frac{1}{4}$	bottom of vertical bar	-	12665
	$\frac{1}{4} \times \frac{1}{4}$	-	-	10950
	$\frac{1}{4} \times \frac{1}{4}$	-	-	11088
	$\frac{1}{4} \times \frac{1}{4}$	-	-	9844
	$\frac{1}{4} \times \frac{1}{4}$	-	-	
$\frac{1}{4} \times \frac{1}{4}$ full size. Scale broke with				10294;
tried again				11006

A prism, having a logarithmic curve for its limits, resembling a column; it was $\frac{1}{4}$ of an inch diameter by one inch long, broke with - - - 6954

April 28th. Trials on prisms of different lengths.

9414.5	$\frac{1}{4} \times \frac{1}{2}$	horizontal	-	9455
	$\frac{1}{4} \times \frac{1}{2}$	ditto	-	9374
	$\frac{1}{4} \times \frac{1}{2}$	ditto, bad trial, 9006 lbs.	-	
9982.5	$\frac{1}{4} \times \frac{1}{2}$	vertical	-	9938
	$\frac{1}{4} \times \frac{1}{2}$	ditto	-	10027

April 29th.

Horizontal Castings.

Averages.					lbs. avoirdupoise.
$\frac{1}{4} \times \frac{3}{8}$	-	-	-	-	9006
$\frac{1}{4} \times \frac{5}{8}$	-	-	-	-	8845
$\frac{1}{4} \times \frac{6}{8}$	-	-	-	-	8362
$\frac{1}{4} \times \frac{7}{8}$	-	-	-	-	6430
$\frac{1}{4} \times \frac{8}{8}$ or one inch long	-	-	-	-	6321

Vertical castings.

$\frac{1}{4} \times \frac{3}{8}$	-	-	-	-	9328
$\frac{1}{4} \times \frac{5}{8}$	-	-	-	-	8385
$\frac{1}{4} \times \frac{6}{8}$	a small defect in the specimen				- 7896
$\frac{1}{4} \times \frac{7}{8}$	-	-	-	-	7018
$\frac{1}{4} \times \frac{8}{8}$ or one inch	-	-	-	-	6430

Experiments on different metals.

$\frac{1}{4} \times \frac{1}{4}$ cast copper, crumbled with	-	-	7318
$\frac{1}{4} \times \frac{1}{4}$ fine yellow brass reduced $\frac{1}{16}$ with	3213.	$\frac{1}{2}$ with	10304
$\frac{1}{4} \times \frac{1}{4}$ wrought copper,	- $\frac{1}{16}$	3427.	$\frac{1}{8}$ 6440
$\frac{1}{4} \times \frac{1}{4}$ cast tin,	- $\frac{1}{16}$	552.	$\frac{1}{3}$ 966
$\frac{1}{4} \times \frac{1}{4}$ cast lead,	-	-	$\frac{1}{2}$ 483

The anomaly between the three first experiments on $\frac{1}{8}$ cubes, and the two second of a different length, can only be accounted for, on the difficulty of reducing such small specimens to an equality. The experiments on $\frac{1}{8}$ inch prisms of different lengths give no ratio. The experiments on $\frac{1}{4}$ inch cubes, taking an average of the three first in each, give a proportion between them and the three on $\frac{1}{8}$ cubes,

as 1 : 6.096 in the block castings

as 1 : 7.352 in the horizontal ditto

as 1 : 8.035 in the vertical ditto

in several cases the proportion is as the cubes.

The vertical cube castings are stronger than the horizontal cube castings.

The prisms usually assumed a curve similar to a curve of the third order, previous to breaking.

The experiments on the different metals, give no satisfactory results. The difficulty consists in assigning a value to the different degrees of diminution. When compressed beyond a certain thickness, the resistance becomes enormous.

Experiments on the suspension of bars.

The lever was used as in the former case, but the metals were held by nippers, as indicated in the drawing No. 2. They were made of wrought iron, and their ends adapted to receive the bars, which, by being tapered at both extremities, and increasing in diameter from the actual section (if I may so express it), and the jaws of the nippers being confined by a hoop, confined both. The bars, which were six inches long, and $\frac{1}{4}$ square, were thus fairly and firmly grasped.

April 30th, 1817.

			lbs.
No. 45	$\frac{1}{4}$ inch, cast iron bar, horizontal	-	1166
46	$\frac{1}{4}$ do. do. vertical	-	1218
47	$\frac{1}{4}$ do. cast steel previously tilted	-	8391
48	$\frac{1}{4}$ do. blister steel, reduced per hammer		8322
49	$\frac{1}{4}$ do. shear steel, do. do.	-	7977
50	$\frac{1}{4}$ do. Swedish iron, do. do.	-	4504
51	$\frac{1}{4}$ do. English iron, do. do.	-	3492
52	$\frac{1}{4}$ do. hard gun metal, mean of two trials		2273
53	$\frac{1}{4}$ do. wrought copper reduced per hammer	-	2112
54	$\frac{1}{4}$ do. cast copper	-	1192

No. 55 $\frac{1}{4}$ do. fine yellow brass	-	1123
56 $\frac{1}{4}$ do. cast tin	- -	296
57 $\frac{1}{4}$ do. cast lead	- -	114

Remarks on the last experiments.

The ratio of the repulsion of the horizontal cast cubes to the cohesion of horizontal cast bars, is 8.65 : 1.

The ratio of the vertical cast cubes to the cohesion of the vertical cast bars, is as 9.14 : 1.

The average of the bars, compared with the cube, No. 16, is as 10.611 : 1.

The other metals decrease in strength, from cast steel to cast lead.

The stretching of all the wrought bars indicated heat.

The fracture of the cast bars was attended with very little diminution of section, scarcely sensible.

The experiment made by M. PRONY, (which asserts, that by making a slight incision with the file, the resistance is diminished one half) was tried on a $\frac{1}{4}$ inch bar of English iron ; the result was 2920lbs., not a sixth part less.

This single experiment, however, does not sufficiently disprove the authority of that able philosopher, for an incision is but a vague term. The incision I made might be about the 40th part of an inch.

Experiments on the twist of $\frac{1}{4}$ inch bars.

To effect the operation of twisting off a bar, another apparatus was prepared : it consisted of a wrought iron lever two feet long, having an arched head about $\frac{1}{8}$ of a circle, of 4 feet diameter, of which the lever represented the radius, the

centre round which it moved had a square hole made to receive the end of the bar to be twisted. The lever was balanced as before, and a scale hung on the arched head; the other end of the bar being fixed in a square hole in a piece of iron, and that again in a vice. The undermentioned weights represent the quantity of weight put into the scale.

May 30th, 1817.

On twists close to the bearing, cast horizontal.

No.		lbs.	oz.
58	$\frac{1}{4}$ in bars, twisted as under with	10	14
59	$\frac{1}{4}$ do. bad casting	-	8
60	$\frac{1}{4}$ do.	-	11

average 9 15

Cast vertical.

61	$\frac{1}{4}$	-	-	10	8
62	$\frac{1}{4}$	-	-	10	13
63	$\frac{1}{4}$	-	-	10	11
				10	10

On different metals.

64	Cast steel	-	-	17	9
65	Shear steel	-	-	17	1
66	Blister steel	-	-	16	11
67	English iron, wrought	-	-	10	2
68	Swedish iron, wrought	-	-	9	8
69	Hard gun metal	-	-	5	0
70	Fine yellow brass	-	-	4	11
71	Copper, cast	-	-	4	5
72	Tin	-	-	1	7
73	Lead	-	-	1	0

On twists of different lengths.

No.	Horizontal.	Weight in scale.
74 $\frac{1}{4}$ by $\frac{1}{2}$ long	- - -	7 3
75 $\frac{1}{4}$ by $\frac{3}{4}$ do.	- - -	8 1
76 $\frac{1}{4}$ by 1 inch do.	- - -	8 8
	Vertical.	
77 $\frac{1}{4}$ by $\frac{1}{2}$ do.	- - -	10 1
78 $\frac{1}{4}$ by $\frac{3}{4}$ do.	- - -	8 9
79 $\frac{1}{4}$ by 1 inch do.	- - -	8 5

Horizontal twists at 6 from the bearing.

80 $\frac{1}{4}$ by 6 inches long	- - -	10 9
81 $\frac{1}{4}$ by do. do.	- - -	9 4
82 $\frac{1}{4}$ by do. do.	- - -	9 7

Twists of $\frac{1}{2}$ inch square bars, cast horizontally.

	qrs.	lbs.	oz.	
83 $\frac{1}{2}$ close to the bearing	3	9	12	end of the bar hard.
84 $\frac{1}{2}$ do.	-	2	18	o middle of the bar.
85 $\frac{1}{2}$ at 10 inches from bearing,	} 1 24 o			
lever in the middle				

On twists of different materials.

These experiments were made close to the bearing, and the weights were accumulated in the scale until the substances were wrenched asunder.

86 Cast steel	- - -	19 9
87 Shear steel	- - -	17 1
88 Blister steel	- - -	16 11
89 English iron, No. 1:	- - -	10 2

No.				Weight in scale.
90	Swedish iron	-	-	9 8
91	Hard gun metal	-	-	5 0
92	Fine yellow brass	-	-	4 11
93	Copper	-	-	4 5
94	Tin	-	-	1 7
95	Lead	-	-	1 0

Remarks.

Here the strength of the vertical bars still predominates.

The average of the two taken conjointly, and compared with a similar case of $\frac{1}{2}$ inch bars, gives the ratio as the cubes, as was anticipated.

In the horizontal castings of different lengths, the balance is in favour of the increased lengths ; but in the vertical castings, it is the reverse. In neither is there any apparent ratio. In the horizontal castings at 6 inches from the bearing, there is a visible increase, but not so great as when close to the bearing.

June 4th, 1817. Miscellaneous experiments on the crush of one cubic inch.

No.		lbs. avoirdupoise.
96	Elm	1284
97	American pine	1606
98	White deal	1928
99	English oak, mean of two trials	3860
100	Ditto, of 5 inches long, slipped with	2572
101	Ditto, of 4 inches do.	5147
102	A prism of Portland stone 2 inches long	805
103	Ditto, statuary marble	3216
104	Craig Leith	8688

In the following experiments on stones, the pressure was communicated through a kind of pyramid, the base of which rested on the hide leather, and that, on the stone. The lever pressed upon the apex of the pyramid. Cubes of one and a half inch.

		specific gravity.	lbs. avoird.
105	Chalk - - -	-	1127
106	Brick of a pale red colour -	2.085	1265
107	Roe stone, Gloucestershire -	-	1449
108	Red brick, mean of two trials -	2.168	1817
109	Yellow face baked Hammersmith paviers 3 times		2254
110	Burnt do. mean of two trials -	-	3243
111	Stourbridge or fire brick -	-	3864
112	Derby grit, a red friable sand stone	2.316	7070
113	Ditto, from another quarry -	2.428	9776
114	Killaly white freestone, not stratified	2.423	10264
115	Portland - - -	2.428	10284
116	Craig Leith, white freestone -	2.452	12346

June 5th, 6th, and 7th, 1817.

117	Yorkshire paving with the strata	2.507	12856
118	Ditto, do. against the strata -	2.507	12856
119	White statuary marble not veined	2.760	13632
120	Bramley Fall sand stone, near Leeds, with strata - - -	2.506	13632
121	Ditto, against the strata -	2.506	13632
122	Cornish granite - - -	2.662	14302
123	Dundee sand stone or Brescia, two kinds - - -	2.530	14918
124	A two inch cube of Portland -	2.423	14918

No.		specific gravity.	lbs. avoird.
125	Craig Leith with the strata	- 2.452	15560
126	Devonshire red marble, variegated		16712
127	Compact limestone	- 2.584	17354
128	Peterhead granite hard close grained		18636
129	Black compact limestone, Limerick	2.598	19924
130	Purbeck	- 2.599	20610
131	Black Brabant marble	- 2.697	20742
132	Very hard freestone	- 2.528	21254
133	White Italian veined marble	- 2.726	21783
134	Aberdeen granite, blue kind	- 2.625	24556

N. B. The specific gravities were taken with a delicate balance, made by CREIGHTON of Glasgow, all with the exception of two specimens which were by accident omitted.

Remarks.

In observing the results presented by the preceding table, it will be seen that little dependence can be placed on the specific gravities of stones, so far as regards their repulsive powers, although the increase is certainly in favour of their specific gravities. But there would appear to be some undefined law in the connection of bodies, with which the specific gravity has little to do. Thus, statuary marble has a specific gravity above Aberdeen granite, yet a repulsive power not much above half the latter. Again, hardness is not altogether a characteristic of strength, inasmuch as the limestones, which yield readily to the scratch, have nevertheless a repulsive power approaching to granite itself.

It is a curious fact in the rupture of amorphous stones, that pyramids are formed, having for their base the upper side of the cube next the lever, the action of which displaces the

sides of the cubes, precisely as if a wedge had operated between them. I have preserved a number of the specimens, the sides of which, if continued, might cut the cubes in the direction of their diagonals.

Experiments made on the transverse strain of cast bars, the ends loose. June 8th, 1817.

		Weight of the		Dist. of bearings		lbs.	
		bars. lbs.	oz.	ft.		avoir.	
135	Bar of 1 inch square -	10	6	3	0	897	
136	{ Do. of 1 inch, do. -	9	8	2	8	1086	
137	{ half the above bar -	-	-	1	4	2320	
138	{ Bar of 1 inch square, through the diagonal -	2	8	2	8	851	
139	{ Half the above bar -	-	-	1	4	1587	
140	{ Bar of 2 in. deep, by $\frac{1}{2}$ inch thick	9	5	2	8	2185	
141	{ Half the above bar -	-	-	1	4	4508	
142	{ Bar 3 in. deep, by $\frac{1}{3}$ inch thick	9	15	2	8	3588	
143	{ Half the bar -	-	-	1	4	6854	
144	Bar 4 inches, by $\frac{1}{4}$ inch thick -	9	7	2	8	3979	
145	Equilateral triangles with the angle up and down.						
146	{ Edge or angle up -	9	11	2	8	1437	
147	{ ——— angle down -	9	7	2	8	840	
148	{ Half the first bar -	-	-	1	4	3059	
149	{ Half the second bar -	-	-	1	4	1656	
150	A feather-edged or \perp bar was cast whose dimensions were						
151	{ 2 inches deep by 2 wide	10	0	edge up	2	8	3105
152	{ Half of ditto						

N. B. All these bars contained the same area, though differently distributed as to their forms.

Experiments made on the bar of $\frac{1}{4}$ inches deep by $\frac{1}{4}$ inch thick, by giving it different forms, the bearings at 2 feet 8 inches, as before.

	lbs.	lbs.
153 Bar formed into a semi-ellipse, weighed	7	4000
154 Ditto, parabolic on its lower edge	-	3860
Ditto, of $\frac{1}{4}$ inches deep by $\frac{1}{4}$ inches thick	-	3979

Experiments on the transverse strain of bars, one end made fast, the weight being suspended at the other, at 2 feet 8 inches from the bearing.

155 An inch square bar bore	-	-	280
156 A bar 2 inches deep, by $\frac{1}{2}$ an inch thick	-	-	539
157 An inch bar, the ends made fast	-	-	1173

The paradoxical experiment of EMERSON was tried, which states that by cutting off a portion of an equilateral triangle (see page 114 of EMERSON'S Mechanics) the bar is stronger than before, that is, a part stronger than the whole. The ends were loose at 2 feet 8 inches apart as before. The edge from which the part was intercepted, was lowermost, the weight was applied on the base above, it broke with 1129 lbs., whereas, in the other case it bore only 840lbs.

Remarks on the transverse strain.

BANKS makes his bar from the cupola, when placed on bearings 3 feet asunder, and the ends loose, to bear 864lbs

Now all my bars were cast from the cupola, the difference was therefore - - - 33lbs.

I adopted a space of 2 feet 8 inches asunder, as being more convenient for my apparatus. The strength of the different bars, all cases being the same, approaches nearly to the

theory, which makes the comparative values as the breadths multiplied into the squares of the depths. The halves of the bars were tried, merely to keep up the analogy. The bar of 4 inches deep, however, falls short of theory by 365 lbs. It is evident we cannot extend the system of deepening the bar much farther, nor does the theory exactly maintain in the case of the equilateral triangle by - - 243lbs. The diagonal position of the square bar, is actually worse than when laid on its side, contrary to many assertions.

The same quantity of metal in the feather edged bar, was not so strong as in the 4 inch bar.

The semi-elliptical bar, exceeded the 4 inch bar, although taken out of it. The parabolic bar came near it.

The bar made fast at both ends, I suspect must have yielded, although the ends were made fast by iron straps. The experiments from EMERSON, on solids of different forms might be made; but the time and trouble these experiments have already cost, have compelled me to relinquish farther pursuits for the present. If, however, in the absence of better, they are worthy of the indulgence of the Royal Society, it will not only be a consolation to me that my labours merit their attention, but a farther inducement to prosecute the investigation of useful facts, which, even in the present advanced state of knowledge, will yet admit of addition.

I am, with much respect,

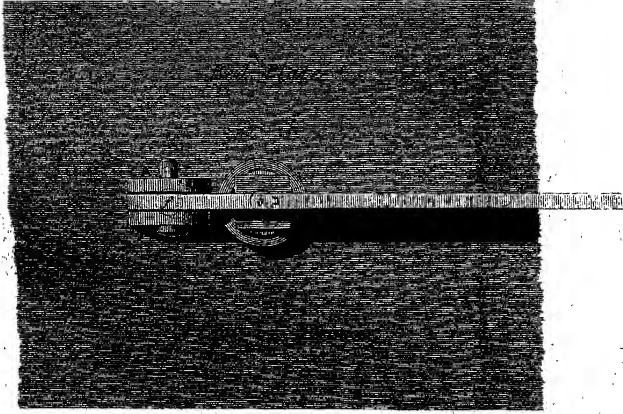
GEORGE RENNIE.

Since my return to England, I find that a set of experiments have been undertaken by Mr. PETER BARLOW, of the

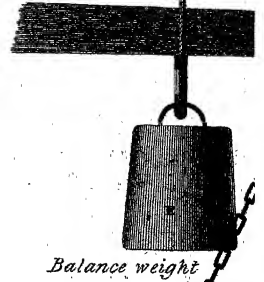
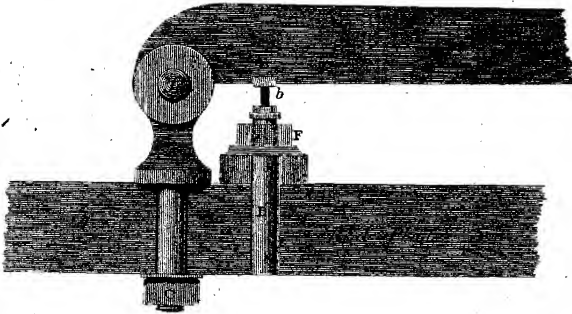
Royal Military Academy. They are very interesting, but contain no experiments on the repulsive power of bodies, and consequently, my communication is not altogether superseded, although a space of seven months has elapsed since this was written.

G. R.

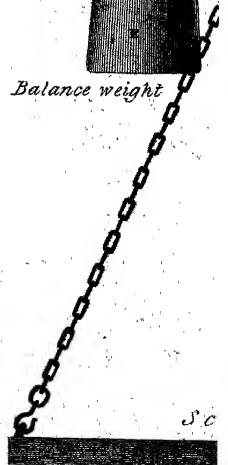
London, Dec. 28, 1817.



The length of the Lever is not shown.



Balance weight

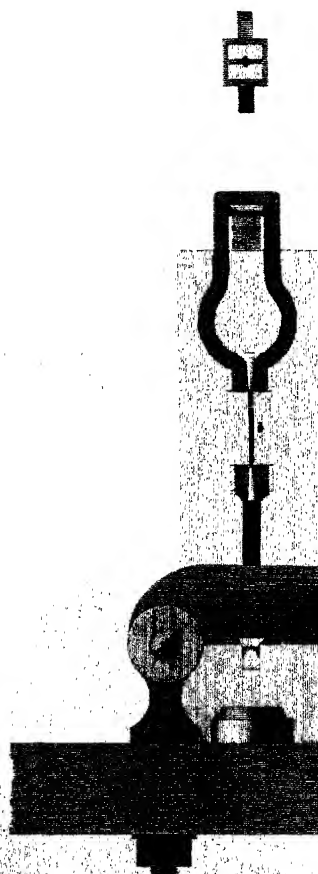


12 11 10 9 8 7 6 5 4 3 2 1 0

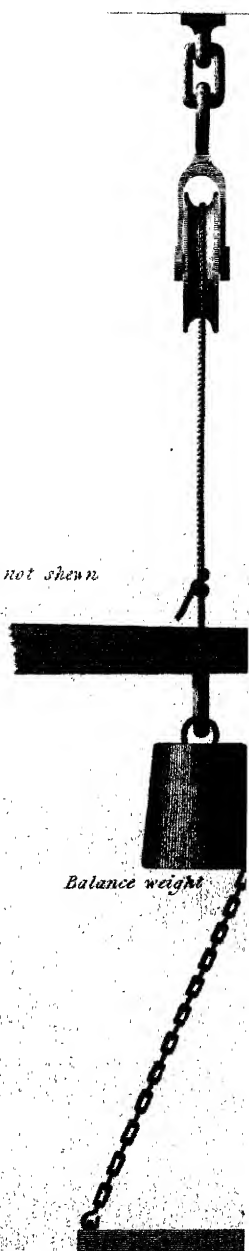
1

Scale of one & a half

Plan of the Nippers



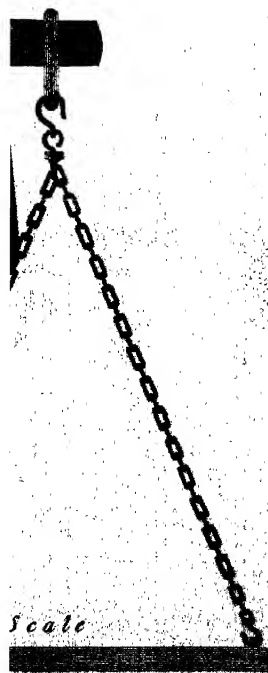
The length of Lever is not shown



Balance weight

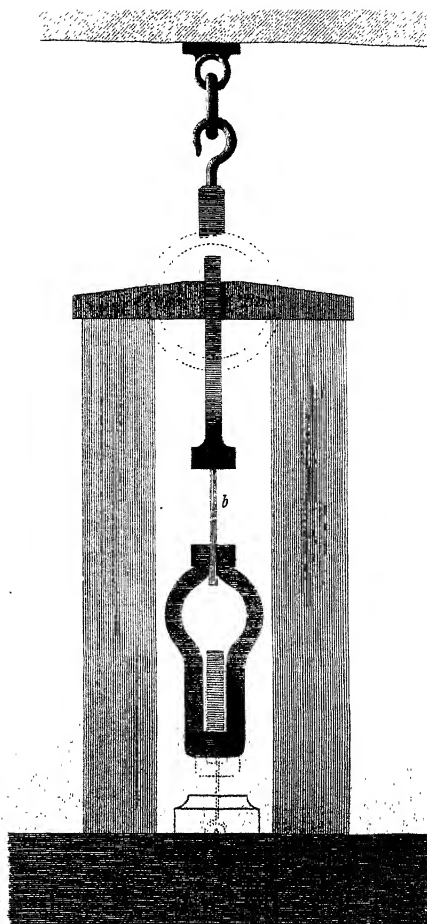


Scale of one ft.

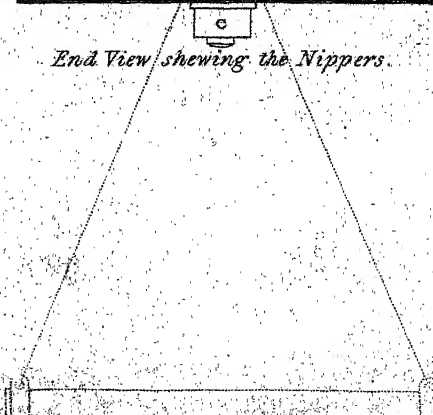


Scale

a half inches to the foot



End View shewing the Nippers.



VIII. *On the office of the heart wood of trees.* By T. A. Knight, Esq. F. R. S. In a Letter addressed to the Rt. Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.

Read February 5, 1818.

MY DEAR SIR,

TREES of every species, that afford timber, live many years before any portion of their alburnum becomes converted into heart wood; and vegetation proceeds with as much vigour previously to the existence of that substance, as subsequently. In the oak it is rarely seen till the seedling tree becomes nearly twenty years old; when it is readily distinguishable from the alburnum by a deeper colour, higher specific gravity, and greater hardness. The tubes also, which extend through the tree longitudinally, and are always open in the alburnum, so as freely to permit the passage of air or water, are closed in the heart wood; and the cellular substance of it has appeared, in every experiment that has come under my observation, to be incapable of conveying the ascending fluid. It does not therefore appear to execute any very important office in the vegetable economy; farther than that it obviously gives, as I have remarked in a former communication, much additional strength to the stem and branches, when these, particularly the latter, become more subject to receive injury, both from the influence of winds and gravitation, on account of the increased distance of their foliage from the points of suspension. Its mode of operation in this case

Pieces of the external, or last formed layer of the alburnum, and of the heart wood, were taken from the trunk (near the ground) of an oak of forty years old, and of very vigorous growth, in the first week of December. These were slowly dried in a temperature not exceeding at any time a hundred and twenty degrees; under which circumstances 1000 parts of the alburnum lost 469 parts, and an equal quantity of the heart wood 500 parts, or precisely half its weight. Upon the 24th of April, similar pieces of the alburnum and heart wood were taken from the same tree, and dried in the same manner; when 1000 parts of the alburnum lost in drying 532 parts, and an equal quantity of the heart wood 507 parts.

The alburnum and heart wood of a poplar tree of about 80 years old were subjected to similar management in December; when 1000 grains of the alburnous substance lost 535 grains, and an equal quantity of the heart wood 626 grains. On the first of March, 1000 grains of alburnum being taken from a tree of similar age and habit with the preceding, lost in drying 557 grains; and the same quantity of heart wood 684 grains. Not only the cellular substance of the heart wood, but the tubes also, which are usually quite empty in the alburnum, were perfectly filled; so that the heart wood of the poplar exhibited nearly the semi-transparency of horn; and in subsequent experience I have found that it contains nearly the same quantity of water in every part of the year.

This abundant fluid in the heart wood was some years ago first observed by M. COULOMB, when felling poplar trees in

the spring. But he conceived it to be merely the fluid, which was ascending from the earth, at that period ; and he concluded from subsequent observation and experiment, in the same season of the year, and in the summer, that the sap of trees chiefly passes up in the vicinity of their medulla, through their heart wood.

M. COULOMB's statements are, I entertain no doubt, perfectly correct ; but the inferences which he, and other continental naturalists have drawn from the facts which he has stated, are, I suspect, erroneous. For I have stated in a former communication, that when I intersected the alburnum of an oak tree in winter, no symptoms of life appeared above such intersection in the ensuing spring. A similar experiment being repeated in the end of June, appeared instantly to intercept the whole of the ascending current, and the leaves of a tree, of which the heart wood remained entire, faded more rapidly than those of another tree of the same species, which was felled at the same period, and lay upon the ground. In the spring of 1816 also, upon the 1st of March, the season of M. COULOMB's experiments, I intersected the alburnum of a poplar tree, 1000 parts of which I have stated to have contained 684 parts water, and an equal quantity of its alburnum 557 parts: yet this tree, notwithstanding the immense quantity of water (probably little less than a ton) which it contained, exhibited very feeble signs of life in the following month, though the weather continued excessively wet ; and before Midsummer it was perfectly lifeless. The elongated cellular, or (as it is usually called) the fibrous texture of the alburnum, through which the sap is now, I

believe, very generally supposed to ascend, appeared to have become impervious upon the conversion of alburnum into heart wood; whilst the lateral or convergent cellular processes remained open to permit the ingress of the moving fluid, without which the heart wood could not probably long retain life.

I must therefore reject the hypothesis which assumes the ascent of the sap through the heart wood, and must believe that the fluid, which affords the organizable matter that is annually deposited in the conversion of alburnum into heart wood, and which subsequently gives greater solidity and strength to that substance, is derived from the bark; and that it passes inwards during the latter part of the summer and autumn, through the convergent cellular (or medullary) processes, to return in part through the same passages when a new layer of bark is to be formed in the spring. Under such circumstances the operation of the heart wood, when it exists in large quantity comparatively with the bark and foliage, as in sound pollard oak trees, must tend to check and diminish, rather than to promote, growth; and amongst trees of this description I have often been able readily to select such as were sound by the slowness of the growth of their branches comparatively with those of other trees of apparently the same age, which were become hollow.

Whether the heart wood of oak trees, which are deprived of their bark in the spring, and suffered to remain standing till the following winter, recover the whole, or a part of the good qualities which it loses (or is supposed to lose) in the spring, is a very interesting question. A few experiments

which I have made, lead me to think it probable, that both the alburnum and heart wood become improved in quality under such circumstances; but I am not at present in possession of such facts as can enable me to give any decisive opinion.

I am, my dear Sir, with great regard,

sincerely yours,

T. A. KNIGHT.

Downton, Dec. 24th, 1817.

IX. *On circulating functions, and on the integration of a class of equations of finite differences into which they enter as coefficients.* By John F. W. Herschel, Esq. F. R. S.

Read February 19, 1818.

(1). **S**o much has been written on the subject of recurring series, and the equations of differences from which they arise, that we can now expect little more to be added to their theory. This is not the case with the class of series, and their corresponding equations I propose to consider in the following pages, which bear a great analogy to the other, and include them as a particular case : I mean, series in which the same relation between a certain number of successive terms recurs periodically ; the terms so related being separated by others connected by relations similar in their general analytical form, but modified by a variation in the constant or variable coefficients which enter into the equations expressing them. Such series have, I believe, never yet been considered as a class : particular cases have very frequently occurred in the course of analytical investigations, and have then been treated by peculiar considerations of such a description as to give a very uninviting air to their theory, but no general view has hitherto been taken of their nature, and no uniform train of analytical artifices been exposed by whose aid they may be subjected to the same modes of treatment as those of the ordinary kind.

(2). Let us imagine a series of quantities

$$u_0, u_1, u_2, u_3, \&c.$$

produced from one another by the following regular law,

$$u_0 = u_0, u_1 = au_0, u_2 = bu_1, u_3 = au_2, u_4 = bu_3, \&c.$$

It is evident then that, were the coefficients a, b , equal, the series would be a recurring one of the simplest kind, viz. a geometric progression, and might be represented by a single equation of differences of the first order

$$u_{x+1} = au_x$$

If this, however, be not the case, the series will consist of two distinct geometric progressions, the terms of which alternate with one another, thus

$$u = u_0 \quad u_2 = abu_0 \quad u_4 = a^2b^2u_0 \quad \&c.$$

$$u_1 = au_0 \quad u_3 = a^2bu_0 \quad u_5 = a^3b^2u_0, \&c.$$

It would seem then that no single equation of differences of the first order could comprehend all the terms of this series, so as to pass uninterruptedly from one to the other; and were this really the case, the method which has hitherto been always followed, of actually resolving it into the distinct series of which it consists, and instituting a separate process for the odd and the even values of x , so as to get the two equations of differences

$$u_{2x+1} = a \cdot u_{2x}, \text{ and } u_{2x+2} = b \cdot u_{2x+1}$$

would be the only course we could pursue. That this, however, is not the case, at least in the instance before us, is evident, if we consider that both these equations are included in the equation of the second order,

$$u_{x+2} = ab \cdot u_x$$

the first integral of which will be an equation of the first order comprehending the whole extent of the series, provided the constant be properly determined by the equation $u_1 = au_0$.

At all events however several inconveniences embarrass this method. It is entirely at variance with the uniformity which ought to reign throughout all analytical operations, thus to descend into arithmetical details in the outset of symbolic investigation, and to vary our processes according to the numerical form of the quantities concerned. If we would avoid this by having recourse to an equation of a higher order including all the separate cases, we are required either to seize at once, by an undirected effort of intellect, on the relation which connects the terms periodically equidistant, or to go through some preparatory process to discover it, which for the most part will be found very troublesome. Moreover, it demands the actual formation of certain terms to determine the constants, which are not (as will hereafter appear) necessary. It is true that in the very simple case I have just stated, these inconveniences, though really existing, are not felt. If we take one a little more complicated, they will speedily form a prominent part of the difficulty. Suppose the law of formation of the terms of a series were, for instance, as follows:

$$u_2 = au_1 + \alpha u_0, u_3 = bu_2 + \beta u_1, u_4 = cu_3 + \alpha u_2,$$

$$u_5 = au_4 + \beta u_3, u_6 = bu_5 + \alpha u_4, u_7 = cu_6 + \beta u_5, \&c.$$

it would be necessary to divide the whole investigation into six cases, and to integrate six several equations of differences,

$$u_{6x+2} = au_{6x+1} + \alpha u_{6x}, u_{6x+3} = bu_{6x+2} + \beta u_{6x+1}, \&c.$$

and after all, the *general term* of the series would not be obtained, but merely the several general terms of six other series which, interlaced, as it were, one with the other, form the series in question; which is in fact much the same way

of proceeding as it would be to consider the series of natural numbers as consisting of several other arithmetical progressions such as

$$\begin{array}{ccccccccccc} 1 & . & . & 4 & . & . & 7 & . & . & 10 & . & . & 13 & . & \&c. \\ 2 & . & . & 5 & . & . & 8 & . & . & 11 & . & . & 14 & . & \&c. \\ 3 & . & . & 6 & . & . & 9 & . & . & 12 & . & . & 15 & . & \&c. \end{array}$$

the general terms of which are respectively $3x - 2$, $3x - 1$, $3x$, united into one, the general term of which is x .

It is interesting then to discover some analytical artifice which shall obviate these inconveniences, by comprehending the whole extent of these and similar series in one single equation, whose order shall be no higher than is absolutely indispensable; which shall require no preparatory investigation to obtain it; nor the actual calculation of any superfluous terms for the determination of the constants in its integral; and, finally, whose integration shall lead to an expression in functions of the index x , such that the substitution of the natural progression of numbers in succession for x shall produce all the terms of the series in their order. Such an artifice, or train of artifices, I shall now proceed to explain. They turn upon a theorem familiar to every algebraist, but which does not seem to have been yet applied to all the uses of which it is susceptible.

(3). Let us then represent by $S_x^{(n)}$ the function

$$\frac{\alpha^x + \beta^x + \gamma^x + \&c.}{n}$$

$\alpha, \beta, \gamma, \&c.$ being the several roots of the equation $z^n - 1 = 0$. If we have occasion to denote other functions similarly composed of the roots of other equations $z^p - 1 = 0$, $z^q - 1 = 0$, &c. they will therefore be represented as follows:

$$S_x^{(p)} = \frac{\alpha^p x + \beta^p x + \&c.}{p}, \quad S_x^{(q)} = \frac{\alpha^q x + \beta^q x + \&c.}{q}, \&c.$$

but when only one quantity n is under consideration, we shall, for convenience, omit the superior index (n) and write the function thus

$$S_x = \frac{\alpha^x + \beta^x + \&c.}{n}$$

Let us also denote by $P_x^{(n)}$ the function

$$a_x \cdot S_x^{(n)} + b_x \cdot S_{x-1}^{(n)} + c_x \cdot S_{x-2}^{(n)} + \dots \dots k_x \cdot S_{x-n+1}^{(n)}$$

omitting, in like manner, the superior index (n) and writing it P_x when only one quantity n is considered. Here we suppose $a_x, b_x, \&c.$ to represent any given functions of x and constant quantities. The functions S_x and P_x are possessed then of the following properties.

(4). S_x is unity whenever x is a multiple of n : in all other cases, $S_x = 0$. This is a well known property of the roots of unity. Hence, some one of the functions

$$S_x, S_{x-1}, S_{x-2}, \dots \dots S_{x-n+1}$$

is necessarily unity, the rest being all zero, though when the numerical form of x is not specified, it is undecided which that one may be. Hence too it follows, that when x is a multiple of n , P_x or

$$a_x \cdot S_x + b_x \cdot S_{x-1} + \dots \dots k_x \cdot S_{x-n+1}$$

reduces itself to a_x ; when $x-1$ is a multiple of n , to b_x when $x-2$ is such a multiple, to c_x , and so on. Thus in all cases P_x will reduce itself to a single term, the form of which will be either $a_x, b_x, \dots \dots k_x$, in rotation, after which the same functions recur over again, for some one of the numbers

$$x, x-1, x-2, \dots x-n+1$$

is necessarily a multiple of n .

If the coefficients $a_x, b_x, \&c.$ be all constant, or if

$$P_x = a \cdot S_x + b \cdot S_{x-1} + \dots k \cdot S_{x-n+1}$$

and we give to x the several values 0, 1, 2, 3, 4, &c. to infinity, in succession, the first n values of P_x will be in their order

$$a, b, c, \dots k$$

after which the same set of quantities will be reproduced in the same order by continuing the substitution, and so on to infinity. The function P_x may be called in this case a *circulating function*, and the same name (with less propriety however) may be extended to the case when the coefficients are variable. The system of coefficients $a, b, c, \dots k$ may be called a period. Hence if the terms of a series be in rotation, $a, b, c, \dots k, a, b, \&c.$ the general term will be truly represented by P_x , or $a S_x + b S_{x-1} + \dots k S_{x-n+1}$, and if they coincide in rotation with the values of n functions $a_x, b_x, \&c.$ thus :

$$a_0, b_1, c_2, \dots k_{n-1}, a_n, b_{n+1}, \&c.$$

the general term will be

$$a_x \cdot S_x + b_x \cdot S_{x-1} + \dots k_x \cdot S_{x-n+1}$$

(5). Any function, however complicated, of $x, S_x, S_{x-1}, \dots S_{x-n+1}$ is reducible to the form P_x

$$\text{Let } \phi(x) = f \{ x, S_x, S_{x-1}, \dots S_{x-n+1} \}$$

be the proposed function. If then x be of the form mn , or a multiple of n , we have $S_x = 1$, and the rest zero, therefore in this case

$$\phi(x) = f \{ x, 1, 0, 0, \dots 0 \}$$

If x be of the form $mn + 1$, or if $x - 1$ be a multiple of n , we have in like manner

$$\phi(x) = f\{x, 0, 1, 0, \dots, 0\}$$

and so on. If then we take

$$a_x = f\{x, 1, 0, 0, \dots, 0\}$$

$$b_x = f\{x, 0, 1, 0, \dots, 0\}$$

$$k_x = f\{x, 0, 0, 0, \dots, 1\}$$

the whole series of values of $\phi(x)$ will coincide, term by term, to infinity, with the corresponding values of

$$P_x = a_x \cdot S_x + b_x \cdot S_{x-1} + \dots + k_x \cdot S_{x-n+1}$$

and therefore $\phi(x) = P_x$, integer values of x only being considered.

For example,

$$a_x = a_x \cdot S_x + a_x \cdot S_{x-1} + \dots + a_x \cdot S_{x-n+1};$$

$$\begin{aligned} \{a_x \cdot S_x + b_x \cdot S_{x-1} + \dots + k_x \cdot S_{x-n+1}\} \times \{a'_x \cdot S_x + b'_x \cdot S_{x-1} + \dots + k'_x \cdot S_{x-n+1}\} \\ = a_x \cdot a'_x \cdot S_x + b_x \cdot b'_x \cdot S_{x-1} + \dots + k_x \cdot k'_x \cdot S_{x-n+1}; \end{aligned}$$

$$\{a \cdot S_x + b \cdot S_{x-1} + \dots + k \cdot S_{x-n+1}\}^m = a^m \cdot S_x + b^m \cdot S_{x-1} + \dots + k^m \cdot S_{x-n+1}.$$

It should be remarked, that any number of the coefficients $a_x, b_x, \&c.$ may be zero, without infringing on the truth of this or the following propositions.

(6). Every symmetrical function of $S_x, S_{x-1}, \dots, S_{x-n+1}$ is invariable.

Let $f\{\bar{x}, \bar{y}, \bar{z}, \&c.\}$ denote a symmetrical function of any number of elements $x, y, z, \&c.$ (in the manner proposed

by Mr. BABBAGE in the Philosophical Transactions for 1816) and let us consider the symmetrical function

$$f\{\bar{S}_x, \bar{S}_{x-1}, \dots, \bar{S}_{x-n+1}\}$$

Then, by the nature of the roots of unity as demonstrated in most treatises on algebra, we have

$$S_x = S_{x-n} = S_{x-2n} = \&c.$$

$$S_{x-1} = S_{x-n+1} = S_{x-2n+1} = \&c. \text{ and so on.}$$

If then x be changed to $x+1$, the above function will become

$$f\{\bar{S}_{x-n+1}, \bar{S}_x, \dots, \bar{S}_{x-n+2}\}$$

that is, by the nature of symmetrical functions, (the order in which the elements are operated on making no difference in their form)

$$f\{\bar{S}_x, \bar{S}_{x-1}, \dots, \bar{S}_{x-n+2}, \bar{S}_{x-n+1}\}$$

the same as before. The function therefore remains unaltered, while x changes to $x+1$, and is therefore invariable.

Hence we have in general

$$\{\bar{S}_x, \bar{S}_{x-1}, \dots, \bar{S}_{x-n+1}\} = f\{\bar{1}, \bar{0}, \dots, \bar{0}\}$$

This proposition may be extended to any number of functions of the form $S_x^{(n)}$, $S_x^{(m)}$, &c. and to functions symmetrical in different senses relative to each set of them: thus we have

$$f\{\overline{S_x^{(n)}}, \overline{S_{x-1}^{(n)}}, \dots, \overline{S_{x-n+1}^{(n)}}, \overline{S_x^{(m)}}, \dots, \overline{S_{x-m+1}^{(m)}}, \overline{S_x^{(p)}}, \dots, \overline{S_{x-p+1}^{(p)}}, \&c.\} \\ = f\{\bar{1}, \bar{0}, \dots, \bar{0}, \bar{1}, \bar{0}, \bar{1}, \bar{0}, \dots, \bar{0}, \&c.\}$$

the accents pointing out the different kinds of symmetry observed in the composition of the function relative to its several elements.

(7). If P_x denote any circulating function with constant coefficients, as

$$a S_x + b S_{x-1} + \dots k S_{x-n+1}$$

Then, 1st, $P_x = P_{x-n} = P_{x-2n} = \&c.$ and 2dly,

$$f \{ \overline{P}_x, \overline{P}_{x-1}, \dots \overline{P}_{x-n+1} \} = f \{ \overline{a}, \overline{b}, \dots \overline{k} \}$$

For in the expression of P_x write successively $x, x-1, x-2, \dots x-n$ for x , and we have (since $S_{x-n} = S_x, \&c.$)

$$P_x = a S_x + b S_{x-1} + \dots k S_{x-n+1}$$

$$P_{x-1} = a S_{x-1} + b S_{x-2} + \dots k S_x$$

$$P_{x-2} = a S_{x-2} + b S_{x-3} + \dots k S_{x-1}$$

$$\left. \begin{aligned} &\dots \dots \dots \\ P_{x-n+1} &= a S_{x-n+1} + b S_x + \dots k S_{x-n+2} \end{aligned} \right\}; (A)$$

and lastly $P_{x-n} = a S_x + b S_{x-1} + \dots k S_{x-n+1} = P_x$.

Any function then, symmetrical relative to all the first members of the equations (A) will, by reason of the circulating form of their second members be also symmetrical relative to $S_x, S_{x-1}, \dots S_{x-n+1}$, whence by the last proposition the truth of this becomes evident. For example

$$P_x \cdot P_{x-1} \dots P_{x-n+1} = a \cdot b \dots k.$$

$$P_x + P_{x-1} + \dots P_{x-n+1} = a + b + \dots k.$$

(8). We are now prepared to proceed to the integration of circulating equations, and to determine by that means the general terms of such series as depend on them. The manner of reducing the law of a series of this kind into an equation will be rendered evident by a simple instance. Suppose this law to be as follows:*

* This example is selected as having actually occurred in an enquiry of another nature.

$$u_2 = au_1 + u_0, u_3 = bu_2 + u_1, u_4 = au_3 + u_2, \\ u_5 = bu_4 + u_3, \&c.$$

The period of the coefficients of the second terms of all these equations being a, b , if we take S_x to represent the sum of the x th powers of the roots of $z^2 - 1 = 0$, the circulating function $aS_x + bS_{x-1}$ will express the general term of the series $a, b, a, b, \&c.$ and the equation

$$u_x = (aS_x + bS_{x-1})u_{x-1} + u_{x-2}$$

will coincide in succession with each of the given ones, by giving x every integer value from z to infinity. This then is the equation of the series, which it only remains to integrate. To this end assume

$$u_x = v_x \cdot \sqrt{aS_x + bS_{x-1}} = v_x \cdot \sqrt{P_x}$$

putting P_x for $aS_x + bS_{x-1}$. Then, by (7) we have $P_{x-2} = P_x$ and the equation becomes, by substitution,

$$v_x \cdot \sqrt{P_x} = P_x \cdot \sqrt{P_{x-1}} \cdot v_{x-1} + v_{x-2} \cdot \sqrt{P_x}$$

that is,

$$v_x = v_{x-1} \cdot \sqrt{P_x \cdot P_{x-1}} + v_{x-2}$$

but, $\sqrt{P_x \cdot P_{x-1}}$ being a symmetrical function of P_x and P_{x-1} is by (7) invariable, and equal to \sqrt{ab} , whence we have

$$v_x = \sqrt{ab} \cdot v_{x-1} + v_{x-2}$$

an ordinary equation with constant coefficients, and consequently integrable by the usual methods. Similar considerations will enable us to arrive at the equations of all periodical series, and we shall therefore confine our attention to the equations alone, in the most extended form of which they are susceptible.

(9). Given the circulating equation

$$u_x + {}^1P_x \cdot u_{x-1} + {}^2P_x \cdot u_{x-2} + \dots + {}^mP_x \cdot u_{x-m} = {}^{m+1}P_x$$

where 1P_x , 2P_x , &c. are any circulating functions, with either constant or variable coefficients, the period of their circulation being the same in each, and equal to n ; to integrate it

$$\text{Assume } u_x = {}^1A_x \cdot S_x + {}^2A_x \cdot S_{x-1} + \dots + {}^nA_x \cdot S_{x-n+1}$$

Then we have

$$u_{x-1} = {}^nA_{x-1} \cdot S_x + {}^1A_{x-1} \cdot S_{x-1} + \dots + {}^{n-1}A_{x-1} \cdot S_{x-n+1}$$

$$u_{x-2} = {}^{n-1}A_{x-2} \cdot S_x + {}^nA_{x-2} \cdot S_{x-1} + \dots + {}^{n-2}A_{x-2} \cdot S_{x-n+1}$$

and so on; and supposing

$${}^1P_x = {}^1a_x \cdot S_x + {}^1b_x \cdot S_{x-1} + \dots + {}^1k_x \cdot S_{x-n+1}, \&c.$$

we shall have by (5),

$$u_{x-1} \cdot {}^1P_x = {}^1a_x \cdot {}^nA_{x-1} \cdot S_x + {}^1b_x \cdot {}^1A_{x-1} \cdot S_{x-1} + \dots$$

similarly,

$$u_{x-2} \cdot {}^2P_x = {}^2a_x \cdot {}^{n-1}A_{x-2} \cdot S_x + {}^2b_x \cdot {}^nA_{x-2} \cdot S_{x-1} + \dots$$

$${}^2k_x \cdot {}^{n-2}A_{x-2} \cdot S_{x-n+1}, \&c. = \&c.$$

The equation then will become by substitution

$$0 = S_x \{ {}^1A_x + {}^1a_x {}^nA_{x-1} + {}^2a_x {}^{n-1}A_{x-2} + \dots$$

$$\dots + {}^ma_x \cdot {}^{n-m+1}A_{x-m} - {}^{m+1}a_x \}$$

$$+ S_{x-1} \{ {}^2A_x + {}^1b_x {}^1A_{x-1} + {}^2b_x {}^nA_{x-2} + \dots$$

$$\dots + {}^mb_x \cdot {}^{n-m+2}A_{x-m} - {}^{m+1}b_x \}$$

$$+ \dots$$

$$+ S_{x-n+1} \{ {}^nA_x + {}^1k_x {}^{n-1}A_{x-1} + {}^2k_x {}^{n-2}A_{x-2} + \dots$$

$$\dots + {}^mk_x \cdot {}^{n-m}A_{x-m} - {}^{m+1}k_x \}; (B).$$

n has here been supposed greater than m . If the contrary

its general form) it will be sufficient to state that the final equation for determining nA_x will be of the form

$${}^nA_{x+mn} + {}^1H_x \cdot {}^nA_{x+mn-n} + {}^2H_x \cdot {}^nA_{x+mn-2n} + \dots + {}^mH_x \cdot {}^nA_x = I_x; \quad (D)$$

1H_x , 2H_x , &c. and I_x being certain known functions of x .

The integrable cases of this equation (which is an ordinary one of finite differences, and although of the $m \times n^{th}$ order reducible by a very obvious substitution to the m^{th}) are for the most part those in which all the functions ${}^1P_x, {}^2P_x, \dots, {}^mP_x$ are circulating functions with constant coefficients, while ${}^{m+1}P_x$ may be of any form whatever, variable or constant. In these cases then the proposed circulating equation is integrable, and they comprise nearly all which are of any real utility in the present state of analysis.

The complete integral of the equation (D) involves $m.n$ arbitrary constants; and, since by means of the system (C) all the other unknown functions ${}^1A_x, {}^2A_x, \dots, {}^{n-1}A_x$ may be expressed by linear combinations of nA_x and its successive values, these constants will be involved in each of the several terms of which u_x consists. But as their number exceeds what is necessary for expressing the complete integral of the proposed, whose order is only m , there must exist equations of relation between them to the number of $(n-1).m$, or else some of them must coalesce into one, by some peculiarity in the composition of these functions. But any such relations may be directly investigated by substituting the value of u_x with all its superfluous constants in the proposed equation, and causing the result to vanish. It will be worth while,

however, to examine the integrable cases above alluded to rather more minutely.

If we leave out the consideration of the last term ${}^{m+1}P_x$, which is allowable, because by the well known theory of linear equations, the part of the integral dependent on the last term can be supplied afterwards, we have $I_x = 0$, and the rest of the coefficients of the equation (D) constant. Denoting them then by ${}^1H, {}^2H, \dots {}^mH$, and supposing $\alpha, \beta, \gamma, \&c.$ to be the roots of

$$z^m + {}^1H \cdot z^{m-1} + {}^2H \cdot z^{m-2} + \dots {}^mH = 0$$

let us take s_x to represent the function

$$\frac{({}^n\sqrt{\alpha})^x + ({}^n\sqrt{\beta})^x + ({}^n\sqrt{\gamma})^x + \&c.}{m}$$

and the integral of the equation

$${}^nA_{x+mn} + {}^1H \cdot {}^nA_{x+mn-n} + \dots {}^mH \cdot {}^nA_x = 0$$

may be universally expressed as follows:

$$\begin{aligned} {}^nA_x = S_x \{ & {}^1C_1 \cdot s_x + {}^2C_1 \cdot s_{x-n} + {}^3C_1 \cdot s_{x-2n} + \dots {}^mC_1 \cdot s_{x-mn+n} \} \\ & + S_{x-1} \{ {}^1C_2 \cdot s_x + {}^2C_2 \cdot s_{x-n} + \dots {}^mC_2 \cdot s_{x-mn+n} \} \\ & + S_{x-2} \{ {}^1C_3 \cdot s_x + {}^2C_3 \cdot s_{x-n} + \dots {}^mC_3 \cdot s_{x-mn+n} \} \\ & + S_{x-n+1} \{ {}^1C_n \cdot s_x + {}^2C_n \cdot s_{x-n} + \dots {}^mC_n \cdot s_{x-mn+n} \} \end{aligned}$$

This may be proved without difficulty from the known form of the integral of the equation of a recurring series. Now since the values of ${}^1A_x, {}^2A_x, \&c.$ are all expressible by means of nA_x as above described, it is easily seen that these are all in like manner circulating functions of a similar form, and since

$$u_x = {}^1A_x \cdot S_x + {}^2A_x \cdot S_{x-1} + \dots {}^nA_x \cdot S_{x-n+1}$$

we may neglect in the expression of 1A_x all those terms which have not S_x for a multiplier; in that of 2A_x , all but those multiplied by S_{x-1} ; and so on. This is a simplification of considerable moment, and will enable us in any assigned case to dispense with the troublesome and complicated process of determining 1A_x , &c. by elimination from (C) after nA_x is obtained. In fact, if we suppose (*)

$$\begin{aligned} {}^1A_x &= S_x \{ {}^1C_1 \cdot s_x + {}^2C_1 \cdot s_{x-n} + \dots {}^mC_1 \cdot s_{x-mn+n} \} \\ {}^2A_x &= S_{x-1} \{ {}^1C_2 \cdot s_x + {}^2C_2 \cdot s_{x-n} + \dots {}^mC_2 \cdot s_{x-mn+n} \} \\ &\text{\&c.} \end{aligned}$$

the relation between the constants which enter into these expressions may be assigned by merely substituting them in the equations (C), and after writing for s_x its value

$$\frac{1}{m} \{ ({}^n\sqrt{\alpha})^x + ({}^n\sqrt{\beta})^x + \text{\&c.} \}$$

and for s_{x-n} , s_{x-2n} , &c. their values.

$$\frac{1}{m} \left\{ \frac{({}^n\sqrt{\alpha})^n}{\alpha} + \frac{({}^n\sqrt{\beta})^n}{\beta} + \text{\&c.} \right\}, \text{\&c.}$$

equating the coefficients of each separate term $({}^n\sqrt{\alpha})^n$, $({}^n\sqrt{\beta})^n$, &c. to zero. This will give $m \times n$ equations of relation between the constants, but it will be found that m of them are necessary consequences of the others, in virtue of the equations (m in number)

* In these equations the constants 1C_1 , 2C_1 , &c. 1C_2 , &c. are not intended to represent the same with those in the expression above given for nA_x which are denoted by the same letters. They are functions of them, whose form it is of no moment to enquire, it being sufficient for our purpose, that the one system of constants consists of an equal number with the other, viz. $m \times n$.

$$\alpha^m + {}^1H . \alpha^{m-1} + \dots + {}^mH = 0$$

$$\beta^m + {}^1H . \beta^{m-1} + \dots + {}^mH = 0, \&c.$$

The above mentioned equations are therefore equivalent only to $(n - 1) . m$ distinct ones, which as we have already seen, is the number of relations which ought to subsist between them. Hence then, all the constants except those which remain arbitrary may be eliminated from the expression of u_x , and the result will be the complete integral required.

(10). Having thus determined the value of u_x , when the last term of the proposed equation, or ${}^{m+1}P_x$ is zero, if we would then extend the integration to cases where it has any given form, the usual theory of linear equations will afford the requisite formulæ, and their application will be attended with no other embarrassment than what may arise from the integration of explicit functions of the forms

$$\Sigma S_x . f(x) \text{ and } \Sigma P_x . f(x)$$

Into this part of the subject, however, we need not enter at any length, because it may be avoided by pursuing the process originally laid down in (9) without neglecting the last term. We shall confine ourselves to the remark, that both the above expressions are reducible to the form

$$a_x . S_x + b_x . S_{x-1} + \dots + k_x . S_{x-n+1},$$

and to the developement of one interesting case, viz. that in which $f(x) = 1$, or the function to be integrated is simply S_x .

Let us then denote ΣS_x by u_x , and it is remarkable that in the determination of u_x all the ordinary methods are unavailing. It is true that since S_x is of the form

$$\frac{\alpha^x + \beta^x + \gamma^x + \&c.}{n}$$

the direct integration of each term will give

$$u_x = \frac{1}{n} \left\{ \frac{\alpha^x}{\alpha-1} + \frac{\beta^x}{\beta-1} + \&c. \right\} + C$$

but the peculiar values of $\alpha, \beta, \&c.$ (the roots of unity) render this expression useless. Neither is it of any service to regard S_x as the general term of a recurring series whose equation is

$$S_{x+n} = S_x$$

for the general expression of the sum of such a series becomes illusory when applied to this particular case. We shall be successful, however, if we regard u_x as originating from an equation of differences $\Delta u_x = S_x$, or $u_{x+1} - u_x = S_x$, and treat it as a particular case of the equation integrated in (9). Thus if we assume

$$u_x = {}^1A_x \cdot S_x + {}^2A_x \cdot S_{x-1} + \dots \dots {}^nA_x \cdot S_{x-n+1}$$

the system of equations (C) becomes

$${}^2A_{x+1} - {}^1A_x = 1$$

$${}^3A_{x+1} - {}^2A_x = 0$$

$${}^4A_{x+1} - {}^3A_x = 0$$

$$\dots \dots \dots$$

$${}^nA_{x+1} - {}^{n-1}A_x = 0$$

These give

$${}^nA_x = {}^1A_{x+1}, {}^{n-1}A_x = {}^nA_{x+1} = {}^1A_{x+2}, {}^{n-2}A_x = {}^1A_{x+3}, \dots \dots \dots {}^2A_x = {}^1A_{x+n-1}$$

and finally

$${}^1A_{x+n} - {}^1A_x = 1$$

But, since a particular value of u_x suffices to determine the general one, we need only seek a particular value of 1A_x :

now such a one presents itself at once, viz. ${}^1A_x = \frac{x}{n}$ because we have obviously $\frac{x+n}{n} - \frac{x}{n} = 1$. Hence then we obtain

$${}^2A_x = \frac{x}{n} + 1 - \frac{1}{n}$$

$${}^3A_x = \frac{x}{n} + 1 - \frac{2}{n}$$

and so on, from which we obtain for the general value of u_x

$$\begin{aligned} u_x &= \text{Const.} + {}^1A_x \cdot S_x + \dots + {}^nA_x \cdot S_{x-n+1} \\ &= \text{Const.} + \{ S_x + S_{x-1} + \dots + S_{x-n+1} \} \left(\frac{x}{n} + 1 \right) \\ &\quad - \frac{1}{n} \{ 0 \cdot S_x + 1 \cdot S_{x-1} + \dots + (n-1) S_{x-n+1} \} \end{aligned}$$

But by (6) we have $S_x + S_{x-1} + \dots + S_{x-n+1} = 1$, and if we include the independent unit in the arbitrary constant, we find

$$u_x = \sum S_x = \frac{x-0 \cdot S_x - 1 \cdot S_{x-1} - 2 \cdot S_{x-2} - \dots - (n-1) S_{x-n+1}}{n} + C$$

There is something remarkable in this expression. If $x+1$ be put for x , and the integral made to vanish when $x=0$, it expresses the sum of the series

$$S_1 + S_2 + S_3 + \dots + S_x$$

this sum will therefore be represented by

$$\frac{x-0 \cdot S_x - 1 \cdot S_{x-1} - \dots - (n-1) S_{x-n+1}}{n}$$

Now it is evident that this series of terms will contain as many equal to unity, as there are units in the integer part of $\frac{x}{n}$, and all the rest are zero. Here then we have an analytical expression for the integer part of the quotient in the division of any one number, x , by any other, n , without in any way specifying their numerical form; whence also a similar expression for the remainder in the same division is easily

obtained : a proposition which seems likely to be of some service in the theory of numbers.

The same expression may also be obtained by the following considerations. If x be a multiple of n , the integer part of $\frac{x}{n}$ will evidently be represented by $\frac{x}{n}$; if x be of the form $in + 1$, or a multiple of n increased by unity, the same integer part will be represented by $\frac{x-1}{n}$; if of the form $in + 2$ by $\frac{x-2}{n}$, and so on. If then we can devise such a function $f(x)$, that when $x = in$ we shall have $f(x) = 0$, when $x = in + 1$, $f(x) = 1$, when $x = in + 2$, $f(x) = 2$, and so on, i being any integer whatever, it is evident that $\frac{x - f(x)}{n}$ will represent in general the integer part of $\frac{x}{n}$. Now

$$0. S_x^{(n)} + 1. S_{x-1}^{(n)} + 2. S_{x-2}^{(n)} + \dots \dots (n-1). S_{x-n+1}^{(n)}$$

is such a function. I thought it right to mention this, because the observation of this fact (so deduced) was among the first things which led me to the general consideration of circulating equations in the form I have here presented it.

(11). The next case I propose to consider is that of circulating equations, in which the period of circulation is not the same in all the coefficients. This, however, will not detain us long. Suppose

$$u_x + P_x^{(n)}. u_{x-1} + P_x^{(p)}. u_{x-2} + \dots \dots P_x^{(t)}. u_{x-m} = P_x^{(v)}$$

$n, p, q, \dots \dots t, v$, denoting the respective periods of circulation in each of the coefficients. Take N to represent the product of all the numbers $n. p. q. \dots \dots t. v$, divided by their greatest common measure, and $P_x^{(n)}$ may be regarded as a circulating function, whose period of circulation is N , but the

coefficients of which, taken in their order, form $\frac{N}{n}$ subordinate periods within it, each consisting of the n coefficients of $P_x^{(n)}$.

Thus suppose $n = 2$, and $N = 6$ then

$$\begin{aligned} P_x^{(2)} &= a_x \cdot S_x^{(2)} + b_x \cdot S_{x-1}^{(2)} \\ &= a_x \cdot S_x^{(6)} + b_x \cdot S_{x-1}^{(6)} + a_x \cdot S_{x-2}^{(6)} + b_x \cdot S_{x-3}^{(6)} + a_x \cdot S_{x-5}^{(6)} + b_x \cdot S_{x-6}^{(6)} \end{aligned}$$

The identity of these two expressions is easily recognised : when x is a multiple of 6, and therefore of 2, they both reduce themselves to a_x ; when $x-1$ is such a multiple, to b_x ; when $x-2$ is a multiple of 6, $x-2$ is also one of 2, and therefore x is so ; consequently both functions reduce themselves to a_x , and so on for every form of x .

Thus every coefficient of the proposed equation may be reduced to one whose period of circulation is N ; and this being done, the equation comes under the general form integrated in (9). The second series whose law is stated in Art. (2) leads to an equation of this kind. In fact the equation

$$u_x - (a S_x^{(3)} + b S_{x-1}^{(3)} + c S_{x-2}^{(3)}) u_{x-1} - (\alpha S_x^{(2)} + \beta S_{x-1}^{(2)}) u_{x-2} = 0$$

coincides in succession with the whole series of equations there assigned ; and if we write instead of the coefficients of this, the following,

$$\begin{aligned} &a S_x^{(6)} + b S_{x-1}^{(6)} + c S_{x-2}^{(6)} + \alpha S_{x-3}^{(6)} + b S_{x-4}^{(6)} + c S_{x-5}^{(6)} \\ &\alpha S_x^{(6)} + \beta S_{x-1}^{(6)} + a S_{x-2}^{(6)} + \beta S_{x-3}^{(6)} + a S_{x-4}^{(6)} + \beta S_{x-5}^{(6)} \end{aligned}$$

they are reduced to a common period of circulation, and the equation may then be integrated as above. It remains to consider equations with more than one independent variable whose coefficients are circulating functions.

(12). A circulating function with two independent variables may have a double period of circulation, and may in general be represented thus :

$$P_{x,y}^{(m,n)} = a_{x,y}^{(0,0)} S_x^{(m)} S_y^{(n)} + a_{x,y}^{(1,0)} S_{x-1}^{(m)} S_y^{(n)} + a_{x,y}^{(0,1)} S_x^{(m)} S_{y-1}^{(n)} + \\ + a_{x,y}^{(m-1,n-1)} S_{x-m+1}^{(m)} S_{y-n+1}^{(n)}$$

one with three may have a triple one, and its expression will be

$$P_{x,y,z}^{(m,n,r)} = a_{x,y,z}^{(0,0,0)} S_x^{(m)} S_y^{(n)} S_z^{(r)} + a_{x,y,z}^{(1,0,0)} S_{x-1}^{(m)} S_y^{(n)} S_z^{(r)} + \&c.$$

and so on, and these functions possess general properties analogous to those of one variable. To avoid complication we will confine ourselves to the case of two variables, a similar mode of treatment being applicable to any number.

First then, any function whatever of $x, y, S_x^{(m)}, S_y^{(n)}, \&c.$ and their successive values is reducible to the form $P_{x,y}^{(m,n)}$ and thus the sums, products, quotients, or powers of two or more such functions are reducible to a single one, as in the case of one variable.

2dly. Any symmetrical function whatever of the values

$$P_{x,y}^{(m,n)}, P_{x-1,y}^{(m,n)}, P_{x,y-1}^{(m,n)}, \dots, P_{x-m+1,y-n+1}^{(m,n)}$$

is invariable, provided the coefficients of each term in the expression of $P_{x,y}^{(m,n)}$ are so; and it is equal to a function similarly composed of those coefficients.

3dly. Any circulating function of the form $P_{x,y}^{(m,n)}$ may be reduced to another of the form $P_{x,y}^{(M,N)}$, M and N being any multiples of m and n respectively, and thus any number of such functions may be reduced to the same period of circula-

tion. These properties are easily demonstrated as in the case of a single independent variable, and by their aid all circulating equations of partial differences may be cleared of their circulating form.

(13). Let the proposed equation of partial differences be

$$u_{x,y} + {}^{(1,0)}P_{x,y}^{(m,n)} \cdot u_{x-1,y} + {}^{(0,1)}P_{x,y}^{(m,n)} \cdot u_{x,y-1} + \dots \dots \dots \\ \dots \dots \dots {}^{(r-1,s-1)}P_{x,y}^{(m,n)} \cdot u_{x-r,y-s} = P_{x,y}^{(m,n)}$$

To integrate it, we assume

$$u_{x,y} = A_{x,y}^{(0,0)} \cdot S_x^{(m)} S_y^{(n)} + A_{x,y}^{(1,0)} \cdot S_{x-1}^{(m)} S_y^{(n)} + A_{x,y}^{(0,1)} \cdot S_x^{(m)} S_{y-1}^{(n)} + \\ \dots \dots \dots + A_{x,y}^{(m-1,n-1)} \cdot S_{x-m+1}^{(m)} S_{y-n+1}^{(n)}$$

and supposing the general representation of any one of the coefficients as ${}^{(r,s)}P_{x,y}^{(m,n)}$ to be

$${}^{(r,s)}a_{x,y}^{(0,0)} \cdot S_x^{(m)} S_y^{(n)} + {}^{(r,s)}a_{x,y}^{(1,0)} \cdot S_{x-1}^{(m)} S_y^{(n)} + {}^{(r,s)}a_{x,y}^{(0,1)} \cdot S_x^{(m)} S_{y-1}^{(n)} + \&c.$$

Then, if the above expression be substituted for u_x in the proposed equation, it becomes

$$= S_x^{(m)} S_y^{(n)} \cdot \{ A_{x,y}^{(0,0)} + {}^{(1,0)}a_{x,y}^{(0,0)} \cdot A_{x-1,y}^{(m-1,0)} + {}^{(0,1)}a_{x,y}^{(0,0)} \cdot A_{x,y-1}^{(0,n-1)} + \&c. - a_{x,y}^{(0,0)} \} \\ + S_{x-1}^{(m)} S_y^{(n)} \cdot \{ A_{x,y}^{(1,0)} + {}^{(1,0)}a_{x,y}^{(1,0)} \cdot A_{x-1,y}^{(0,0)} + {}^{(0,1)}a_{x,y}^{(1,0)} \cdot A_{x,y-1}^{(1,n-1)} + \&c. - a_{x,y}^{(1,0)} \} \\ + S_x^{(m)} S_{y-1}^{(n)} \cdot \{ A_{x,y}^{(0,1)} + {}^{(1,0)}a_{x,y}^{(0,1)} \cdot A_{x-1,y}^{(m-1,1)} + {}^{(0,1)}a_{x,y}^{(0,1)} \cdot A_{x,y-1}^{(0,0)} + \&c. - a_{x,y}^{(0,1)} \} \\ \dots \dots \dots \\ + S_{x-m+1}^{(m)} S_{y-n+1}^{(n)} \cdot \{ A_{x,y}^{(m-1,n-1)} + {}^{(1,0)}a_{x,y}^{(m-1,n-1)} \cdot A_{x-1,y}^{(m-2,n-1)} + \&c. - a_{x,y}^{(m-1,n-1)} \}$$

Let now each term of this, enclosed in the brackets, be separately made to vanish, and we shall obtain a system of *re-entering equations* of partial differences, for the determination of the unknown functions $A_{x,y}^{(0,0)}$, &c. The number of these equations is $m \times n$, which being also that of the functions to be determined, they suffice for the purpose.

It will be unnecessary to enter into the detail of the process of elimination in this case; it is always practicable, and leads to a final equation with constant coefficients, when those of the circulating functions which enter into the proposed equation are constant, just as in the case of one variable.

(14). I am unwilling to occupy the pages of the *Philosophical Transactions* with examples of the application of the processes here delivered to the various problems in the pure and mixed mathematics where they afford either a remarkable simplicity in the result, or great neatness in the investigation. Such instances occur frequently in the evaluation of continued fractions and other similar functions where the denominators (or other elements) recur in a certain order. A variety of complicated questions relative to the simultaneous employment of capital in different mercantile transactions, can scarcely be treated with perspicuity in any other way, and other instances will readily suggest themselves to the reader whose experience in enquiries of this nature has led him to feel the inconvenience which these pages are designed to obviate. I will therefore merely subjoin one example of the integration of a circulating equation of the second order, with constant coefficients, by way of illustration of the methods themselves.

Suppose then we have

$$u_x - (a S_x + b S_{x-1}) u_{x-1} + (a S_x + \beta S_{x-1}) u_{x-2} = 0$$

where S_x is $\frac{1}{2}$ the sum of the x^{th} powers of the roots of $z^2 - 1 = 0$ assume

$$u_x = A_x \cdot S_x + B_x \cdot S_{x-1}$$

and we get by substitution

$0 = S_x \{ A_x - a B_{x-1} + \alpha A_{x-2} \} + S_{x-1} \{ B_x - b A_{x-1} + \beta B_{x-2} \}$
whence we find

$$A_x + \alpha A_{x-2} = a B_{x-1}$$

$$B_x + \beta B_{x-2} = b A_{x-1}$$

The first of these gives $B_x = \frac{1}{a} \{ A_{x+1} + \alpha A_{x-1} \}$; (E)
which substituted in the second multiplied by a produces

$$A_{x+1} + (\alpha + \beta - ab) A_{x-1} + \alpha \beta A_{x-3} = 0$$

$$\text{or, } A_{x+4} - (ab - \alpha - \beta) A_{x+2} + \alpha \beta A_x = 0$$

This equation corresponds to the equation (D) of the foregoing articles, and if we take p and q such that

$$(z^2 - p^2)(z^2 - q^2) = z^4 - (ab - \alpha - \beta) z^2 + \alpha \beta,$$

we shall have for the complete value of A_x

$$A_x = S_x \{ C \cdot (p^x + q^x) + C' \cdot (p^{x-2} + q^{x-2}) \} \\ + S_{x-1} \{ c \cdot (p^x + q^x) + c' \cdot (p^{x-2} + q^{x-2}) \} \quad (F)$$

This expression substituted in (E) will give the value of B_x , but as it is sufficiently evident that the expression so obtained, as well as the value of u_x thence derived, will all have the same form, the constants only differing, we may at once suppose u_x equal to the second member of (F) and determine the relations between C, C', c, c' by substituting it in the proposed equation. This gives the following four equations of condition among the constants.

$$\left. \begin{aligned} (C + \frac{C'}{p^2})(1 + \frac{\alpha}{p^2}) - \frac{a}{p}(c + \frac{c'}{p^2}) &= 0 \\ (c + \frac{c'}{p^2})(1 + \frac{\beta}{p^2}) - \frac{b}{p}(C + \frac{C'}{p^2}) &= 0 \end{aligned} \right\}$$

$$\left. \begin{aligned} (C + \frac{C'}{q^2})(1 + \frac{\alpha}{q^2}) - \frac{a}{q}(c + \frac{c'}{q^2}) &= 0 \\ (c + \frac{c'}{q^2})(1 + \frac{\beta}{q^2}) - \frac{b}{q}(C + \frac{C'}{q^2}) &= 0 \end{aligned} \right\}$$

Eliminating $(c + \frac{c'}{p^2})$ from the two first of these, and $(c + \frac{c'}{q^2})$ from the two last we find

$$\frac{1}{p^4} \cdot (C + \frac{C'}{p^2}) \cdot \{p^4 - (ab - \alpha - \beta)p^2 + \alpha\beta\} = 0$$

$$\frac{1}{q^4} \cdot (C + \frac{C'}{q^2}) \cdot \{q^4 - (ab - \alpha - \beta)q^2 + \alpha\beta\} = 0$$

but the values of p and q are such by hypothesis, that the latter factors of these equations vanish separately of themselves: the equations are therefore verified independently of any particular values of $C + \frac{C'}{p^2}$ or $C + \frac{C'}{q^2}$, which therefore remain arbitrary. C and C' then being arbitrarily assumed, and c , and c' determined from the above equations, which give

$$c = \frac{Cpq(p^2 + q^2 + pq + \alpha) + C'(pq - \alpha)}{a \cdot pq \cdot (p + q)}$$

$$c' = \frac{C\{(pq)^2 + \alpha(p^2 + q^2 + pq)\} - C \cdot (pq)^2 \cdot (pq - \alpha)}{a \cdot pq \cdot (p + q)}$$

the second member of the equation (F) will be the complete value of u_x , and it will be noticed that we have

$$pq = \sqrt{\alpha\beta}, p^2 + q^2 = ab - (\alpha + \beta)$$

$$(p + q) = \sqrt{(p^2 + q^2) + 2pq}$$

whence the whole is readily reduced to functions of the proposed coefficients a, b, α, β .

JOHN F. W. HERSCHEL.

X. *On the fallacy of the experiments in which water is said to have been formed by the decomposition of Chlorine.* By Sir H. Davy, LL.D. F.R.S.

Read February 12, 1818.

SOME experiments have been lately communicated to the Royal Society of Edinburgh, from which it has been inferred, that water is formed during the action of muriatic acid gas on certain metals, and consequently, that chlorine is decomposed in this operation.

In repeating those experiments, I have ascertained, that the water is derived from sources not suspected by the authors, and that their conclusions are unfounded. To take up the time of the Society by long experimental details and theoretical speculations on such an occasion, will be unnecessary; I shall therefore only transiently mention the sources of error, and demonstrate their operation by two or three examples.

When muriatic acid gas is passed through flint glass tubes heated to redness, a small quantity of water is formed by the action of the gas on the oxide of lead in the glass, and a smaller quantity by its action on the alkali of the glass, the process being one of double affinity, the hydrogen of the muriatic acid unites to the oxygen of the oxide, and the chlorine combines with the metals.

A copious dew was formed by passing muriatic acid gas through flint glass tubes red hot, and a less copious dew, by passing it through green glass tubes. In the first instance,

the glass became opaque, and gained a pearly lustre, and a combination of chlorine and lead sublimed from the hotter into the colder part of the tube. In the second, the surface of the tube became slightly opaque, but no sublimate was formed.

When fine clean iron wire was introduced into such tubes, and made red hot, and muriatic acid gas passed over it, no particular precautions being taken to free the tubes from common air, much more water appeared; but this excess of water principally owed its existence to the combination of hydrogen disengaged from the muriatic acid gas by the iron with the oxygen of the common air. I say, *principally*, because an inappreciable quantity must have been deposited from the vapour of hydrated muriatic acid in the muriatic acid gas. This was proved by filling the whole apparatus with hydrogen in another experiment, and generating the muriatic acid gas in a retort filled with hydrogen, when the water produced was no more than might have been expected from the action of the muriatic acid gas on the oxide of lead and alkali in the glass. I give the details. Above 21 grains of the first combination of chlorine and iron were formed; the quantity of moisture collected by bibulous paper, and which was a strong acid solution of the proto-muriate of iron, amounted to less than half a grain, and of this not more than two-thirds could have been water. Now, if chlorine had been decomposed in this operation, the quantity of water ought to have been at least ten times as great.

I have shown by numerous experiments, that in the action of muriatic acid gas upon metals, hydrogen, equal in bulk to ~~half the volume~~ of the gas, is produced; it is therefore evident, that if water had been generated by the action of mu-

muriatic acid gas on metals, it must have been the *chlorine*, or the *metal*, or both, that were decomposed. As chlorine can be freed from much of its aqueous vapour by dry muriate of lime, which is not the case with muriatic acid gas, it offers a much more unexceptionable substance for experiments of this kind. I passed 23 cubical inches of chlorine slowly through dry muriate of lime into a flint glass tube red hot, containing a green glass tube full of iron wire; the chlorine combined with this iron wire with intense heat; the bright sublimate formed was passed through more iron wire heated to redness, so as to form a considerable quantity of the first compound of chlorine with iron, which, when examined, was found exactly the same as that produced by the action of muriatic acid gas on iron. All the products were heated strongly, and the end of the glass tube kept very cool; but *not the slightest appearance of moisture was perceptible.*

In all these experiments I was assisted by Mr. FARADAY of the Royal Institution.

Muriate of ammonia is not altered by being passed through porcelain or glass tubes heated to redness, but if metals be present, it offers similar results to muriatic acid gas. In one experiment, in which muriate of ammonia recently sublimed was used, instead of muriatic acid gas, the appearance of moisture was less than in the experiment on muriatic acid gas, which has been just detailed, and yet there was a considerable action on the oxide of lead in the glass, not only by the muriatic acid, but likewise by the free hydrogen of the decomposed ammonia.

XI. *The Croonian Lecture. On the changes the blood undergoes in the act of coagulation. By Sir Everard Home, Bart. V. P. R. S.*

Read November 20, 1817.

IT is not a little remarkable, that in the first Lecture of this kind, which I laid before the Society, in the year 1790, I should have endeavoured to show, that a muscular fibre was too minute an object to be seen by the human eye, even assisted by the best magnifying glasses then in use ; and that in this Lecture, I shall be able, by means of the great improvements that have been made in the use of the microscope, to show that a fibre not larger in diameter than one of the globules of the blood can be demonstrated.

To the Members of this Society who have so lately seen Mr. BAUER's drawings, of the glandular apparatus peculiar to the Java swallow ; of the internal membrane of the human stomach, exposing structures that were not known to exist ; also of so small an object as the human ovum, in which is seen the seat of the two most important organs of the body ; (drawings rendered beautiful by their simplicity and distinctness ;) it will readily suggest itself, that Mr. BAUER is the person to whom I consider we are indebted for those improvements. His whole life, I may say, has been employed in investigations of a similar nature in plants, observing first the natural appearances, and then magnifying them in different

degrees, and comparing, with the nicest discrimination, what was exhibited by one magnifying power, with what was shown by that immediately above it, and, where they did not exactly correspond, employing the whole energies of his mind, with a patient labour, almost beyond what is natural, in ascertaining the cause of the deception which must in one of them have taken place. To the observations of such a man upon subjects of this nature, if we are not confidently to place a reliance, how are we to give credit to the remarks that are made by common observers?

I have said thus much as an introduction to the observations that I am going to bring forward, for the public to know, whatever opinion they may form of them, they have been the result of long and unwearied research; and have been so frequently repeated as to satisfy Mr. BAUER of their correctness.

The red globules of the blood in the human body, when enveloped in their colouring matter, appear, when measured in the microscope by the micrometer, to be $\frac{1}{1700}$ part of an inch in diameter, requiring 2,890,000 to a superficial or square inch. These globules, when deprived of their colouring matter, appear to be $\frac{1}{2000}$ part of an inch in diameter, which makes 4,000,000 of globules to a square inch. From these observations, it appears that the globules, when deprived of the colouring matter, are not quite one fifth part smaller. The colouring substance appears not to be contained in the globules, but only to envelope them: one reason for forming this opinion is, that the separation is very rapidly effected, the colouring substance flowing from all parts of the globule at the same instant, and that to retain the globules in the coloured state it is necessary that a very small quantity of blood only be

smieared as thin as possible upon the glass, in order that all moisture may instantly evaporate ; they then remain of their full size and colour, perfectly spherical, as in the representation, fig. 1, Pl. viii. But if a greater quantity of blood be laid upon a glass which retains moisture only half a minute, the colouring matter begins, in a few seconds, to separate and form a circle round the globule, and if the blood is diluted with water, the separation of the colouring matter is instantaneous, and the globule puts on the appearance represented in fig. 2, Pl. viii. Another reason is, the great quantity of colouring matter, it being as three to one in proportion to the globules.

The globules of the blood have neither the same size nor the same shape in all animals. Dr. YOUNG, in his introduction to Medical Literature, has described them to be of an oval form in the skate. Upon examining them in that fish, Mr. BAUER found them, while the fish was alive, of the form of an egg, but almost immediately after death, flattened. I shall, however, reserve the materials I have collected upon this subject for another communication. When the globules in the human blood lose the colouring matter, they continue floating in the serum, and are seen to have an attraction towards one another so as to coalesce, uniting themselves together. In the annexed drawing is represented their mode of union under different circumstances, surrounded by serum deeply tinged with the colouring matter. In one instance three globules are so united as to form one line; in another there is a line composed of four globules, with lateral indentations, where the union between the globules had taken place. This appearance, joined to other circumstances, renders it probable at least that the globules may be the part of the blood out of which

the muscular fibres are principally formed, and no fibre they could form would be of smaller dimensions than the globules of which it is composed. Having expressed this opinion to Mr. BAUER, I was desirous that he should try to unravel a muscle so as to come at the ultimate fibre. Several attempts of this kind were unsuccessfully made : but in one instance, in the muscles of the thigh of a roasted chicken, a detached fibre was exposed in the microscope, which occupied upon the micrometer, the same space in every respect with the four globules which I have mentioned to have seen united, and floating in the serum of the blood before the blood had completely coagulated ; the muscular fibre from the chicken could be traced to a greater length, but the indentations could not be distinguished farther on.

In prosecuting this examination of the muscular fibres, Mr. BAUER found that after being boiled or roasted, and then macerated in water, changing the water every day for a week, they were much more readily separated from each other, and that he had no difficulty in procuring single fibres similar to the one described, from the coats of the human stomach ; the thigh of the sheep, and of the rabbit ; and from the salmon. The appearances that different fibres put on are represented in the annexed figures, 4, 5, 6, of Plate viii.

When the fibres are macerated for a longer time, they are readily broken down into a mass of globules of the size of those in the blood, deprived of their colour. The accuracy of the appearances that have been described may be depended on ; how far they will afford the slightest grounds for an opinion that the globules are the materials, and the attraction between them the means, by which the single fibres are formed, and all the

combinations produced that are met with in the structure of muscles, must require farther investigation. It is deserving of remark, that while the globules are enveloped in their colouring matter, they are not seen to run together, and coalesce with one another on the field of the microscope; it is therefore probable that the attraction, by which this effect is produced, only takes place between globules deprived of their colour.

It may not be amiss to enquire how far there is any thing in the formation of fibres in other parts of the body at all in favour of muscular fibres being composed of globules, and I shall mention that Mr. BAUER, in his examination of the substance of the brain, under the microscope, finds, that when that organ is immediately after death made the subject of examination, abundance of fibres are met with in every part of it; indeed it appears that the whole mass is a tissue of fibres, which seems to consist entirely of an accumulation of globules, whose union is of so exceedingly delicate a nature, that the slightest touch, even the mere suction in water, deranges and reduces them to that mass of globules of which the brain appears to be composed when examined with less accuracy, or under less favourable circumstances. He admits that in his first observations he was induced to believe that no such things as fibres were to be met with in the brain, but that the whole organ consisted of a mass of these globules. He found that the globules of the brain, as well as those of pus, are exactly the same size as those of the blood when deprived of their colouring matter.

Mr. BAUER not having completed the investigation of the brain, in which he is engaged, I shall not farther anticipate

his observations upon this most important organ: as my only object in what I have already stated, is to mention, that the fibres of which it appears to be made up are composed of globules.

Upon mentioning Mr. BAUER's observations on the brain to some of my friends, I was referred to the Supplements to the 4th and 5th editions of the *Encyclopædia Britannica*,* in which the opinion of the brain being composed of globules is noticed. I find, upon making such reference, that there is not enough stated respecting the anatomy of the more minute parts of that organ, to supersede a farther investigation of its structure, as will appear from the following extracts.

“ It (the brain) has been subjected to very minute microscopical observations by PROCHASKA.† When he took a small portion of it, either from the brain proper or the cerebellum, and spread it on a thin plate of glass, so that it became pellucid, and then examined it with a powerful microscope, he found that it resembled a sort of pulp, consisting of innumerable globules, or particles of a roundish form. A little water added to this pulp, divided it into a number of flocculi; but he observed that each flocculus was still composed of a number of globules. He very rarely found one globule by itself, or even two, floating in water, apart from the rest. Maceration in water, even for three months, was insufficient to separate them from each other. He concluded, therefore, that they were united by means of a very delicate and pellucid cellular substance. The globules, he observed, were not all of the

* Vol. 1, part 2, page 260, No. 64 and No. 65.

† *Oper. Min. Pars*, I. p. 342.

same size; but varied a little in dimension, even in the same part of the brain. In general, however, he found them, both in the brain proper and in the cerebellum, to be more than eight times smaller than a globule of the blood. The most powerful microscopes did not enable him to discover any thing satisfactory respecting their structure."

" These observations have, within these few years, been prosecuted on a much more extensive scale, by JOSEPH and CHARLES WENZEL.* They have uniformly found, that the white nervous matter seemed as if entirely composed of extremely small globules or corpuscles of a roundish form, putting on the appearance of little cells, filled with a proper medullary substance. No estimate is given of the dimensions of the globules, but they describe them as being exceedingly minute, and as being all pretty nearly of the same size. They seemed to adhere very closely to each other, without any apparent connecting medium. The globular appearance continued distinctly perceptible in portions of the substance, which had been long exposed to the action of rectified spirit of wine and muriatic acid; nor was it even destroyed by steeping the matter in alcohol, and then drying it."

The statements contained in both of these extracts confirm, in the most satisfactory manner, Mr. BAUER's observations, although in many respects, they are deficient in point of accuracy.

Having laid before the Society all I have to offer, respecting the appearances of the globules of the blood from which the colouring matter had been discharged, I shall endeavour to explain in what manner blood, in the act of coagulation,

* De Penitior. Struct. Cereb. p. 24.

acquires that texture, which fits it, when extravasated in living animals, to open a communication with the general circulation, and by that means become a part of the solids of the animal.

It has ever been a desideratum to ascertain in what manner blood after it had coagulated, and remained at rest in different parts of a living animal, is rendered vascular. The fact itself has long been known to every enquirer into the operations of the animal economy, and several theories have been formed to explain it. Mr. HUNTER, who perhaps understood the appearances such coagula put on, when injected from the neighbouring vessels, better than any other physiologist, was unable to trace a direct continuation of ramifying branches from the circumference of the living parts to the centre of the coagulum, and therefore referred it to a principle of life existing in the blood, which principle was consequently inherent in the coagulum, and formed a series of vessels, pervading every part of it, which opened for themselves communications with the surrounding vessels. Since Mr. HUNTER's time, no more satisfactory opinion has been advanced for the explanation of this curious phenomenon. I confess that my own attention has not, for the last twenty years, been called to this enquiry, although before that time, while I was assisting Mr. HUNTER in the prosecution of his pursuits, I gave considerable attention to it, but remained unsatisfied with all the explanations that had been given.

My attention was, however, again called to this subject last summer, by different conversations I had with Mr. BAUER, in which he told me, that to illustrate the germination and vegeta-

tion of wheat, introductory to his illustrations of the diseases in corn, he sowed a quantity of wheat, and afterwards took up every day some grains, or plants, for examination, till the ears were ripe. In his close attention to the changes that took place, he was very much struck with the rapid increase of the tubular hair of the root of a young plant of wheat, in its earliest stage of vegetation: and fixing his whole attention upon that part of the plant, he observed small pustules of a slimy substance arising under the epidermis, on the surface of the young root; and, in a few seconds, a small bubble of gas burst from the root into the slimy matter, which it extended in a moment to the length the hair was to acquire; and the slimy matter, surrounding the gas, immediately coagulated, and formed a canal. He repeated his observations on another plant, whose pubescence consisted of a jointed hair, and observed the same effect take place; a bubble issued from the young stalk, and extended the slimy mucus to a short distance, forming the first joint, which immediately coagulated and became transparent, and at its extremity a new pustule of the same slimy mucus accumulated, into which, in a short time, the gas from the first joint rushed; and thus, in a moment, a second joint was formed: in the same manner he observed the formation of the hairs of ten or twelve joints to take place.

These observations, so curious in themselves, and which explain, in so simple and satisfactory a manner, one of the modes in which tubes are formed in vegetables, and an addition is made to the plant, made so strong an impression on my mind, and so entirely engrossed my attention, that I did

not allow Mr. BAUER to rest, till he gave me his assistance in instituting experiments, to ascertain whether any thing similar takes place in animal bodies.

The first object of our enquiry was to know, whether any gas is to be found in the blood while circulating in the vessels, and under what circumstances it is separated from it. That the blood, whilst circulating in the arteries and veins, holds a considerable quantity of gas in solution, is proved by the following experiments, made at my request, by Professor BRANDE. Blood was drawn from a vein in the arm, and whilst yet warm was placed under the receiver of an air pump; during the exhaustion of the receiver, there was a considerable escape of gas from the blood, so that it had the appearance of effervescing, and soon depressed the quicksilver in the gage of the pump. He afterwards ascertained that this gas is carbonic acid gas, is met with in the same proportions in arterial and venal blood, and two cubic inches were extricated from every ounce of blood.*

That a considerable portion of this carbonic acid gas, is extricated from the blood during the spontaneous coagulation of that fluid, was previously proved by Mr. BAUER, who filled glass tubes with blood recently drawn, and tying them over with bladder, inverted them. At first there was no appearance of gas upon the surface, but as the blood coagulated, it was separated, and in the course of 24 hours was found in considerable quantity.

Having ascertained not only the existence of gas in the blood, but that it is separated during the process of coagulation, I was most anxious to discover whether, as in vegetables, the gas,

* See *Annales de Chimie*, Tom. XIII.

thus let loose, pervaded the surrounding fluid into which it is propelled, in any particular manner. With a view to determine this point, I wounded the skin of my arm with the point of a lancet, so as to draw a drop of blood, which was received into a watch glass in a fluid state, and placed in the field of the microscope. The eye was then kept constantly fixed upon it, to watch the changes that might take place. The first thing that happened, was the formation of a film upon the surface, that part beginning to coagulate sooner than the rest. In about five minutes, something was seen to be disengaged in different parts of the coagulum, beginning to show itself where the greatest number of globules were collected; and from thence passing in every direction with considerable rapidity through the serum, but not at all interfering with the globules themselves, which had all discharged their colouring matter; wherever this extricated matter was carried, a net-work immediately formed, anastomosing with itself on every side, through every part of the coagulum. When the parts became dry, the appearance of a net-work remained unaltered. In some instances, bubbles were seen to burst through the upper surface of the coagulum: this, however, did not prevent the ramifications that have been described from taking place. The annexed Plates give the exact representation of the appearance the blood taken from my arm put on, when it coagulated and became dry, as shown in the field of the microscope. [Pl. ix, x.]

If the blood is cold when it is exposed in the microscope, and there is a larger quantity of serum upon the glass, the net-work is only formed in those parts where clusters of globules are collected; and when the serum dries, it cracks, and spoils.

the appearance. This happens sometimes several days after the formation of the net-work has taken place. When clear serum without any globules is put upon the glass, nothing is extricated, but when the serum is quite dry, it cracks, and the cracks may be mistaken for the net-work; but by comparing them with it, the difference is found to be obvious.

These facts which Mr. BAUER has enabled me to bring forward, appear to point out an important change the blood undergoes, after it is extravasated. When this happens in living animal bodies, from whatever cause, and in whatever circumstances it takes place, no difficulty remains in accounting for its afterwards becoming vascular, since all that is necessary for that purpose is the red blood being received into the channels of which this net-work is formed.

I shall not detain the Society with any farther observations in support of what I have advanced, satisfied that if the facts do not bear themselves out, it will be superfluous to load them with theoretical opinions.

I cannot conclude this Lecture, without paying a tribute to the President of the Society; not a tribute of praise, but a tribute of justice; for whenever general science, or any of its branches, are brought under his consideration, the zeal and exertion, which he shows upon all occasions, to promote the pursuits of individuals, exceeds whatever has been done by others, in this or any other country, and is above all praise.

Whatever Mr. BAUER has already done, and whatever he may hereafter bring to light, respecting the more minute parts of animal bodies, is entirely to be attributed to the President; for it is at his particular request, under his encouragement, and in compliance with his wishes, that Mr.

BAUER has put a restraint upon himself, and for a time laid aside the prosecution of the Anatomy of Vegetables, which from his early youth has been his favourite occupation, to assist in bringing to light appearances in the anatomy of animals, which, without his aid, must still have remained in obscurity.

XII. *Some additions to the Croonian Lecture, on the changes the blood undergoes in the act of coagulation.* By Sir EVERARD HOME, Bart. V.P.R.S.

Read March 5, 1818.

SEVERAL of my friends, much more deeply versed in mathematics than myself, who were present at the reading of the Croonian Lecture, remarked that no spherical bodies could be accurately measured by the common micrometer, and therefore no correct idea of the diameter of a globule of the blood could be obtained by that means. They were also led to doubt the appearance represented in the coagulum, being real, since air, in all ordinary circumstances, when let loose, forms itself into globules, not moving in straight or curved lines.

These objections, coming from philosophers for whose opinions on such subjects I have the highest respect, induced me to request permission of the President to withdraw the Lecture, that I might correct any errors I had fallen into before the Paper came before the Committee. I found also upon reflection, that I had left the investigation more imperfect than I was aware of, since it is of very little consequence whether, in the act of drying, coagulated blood puts on this particular appearance or not, if I cannot at the same time adduce proofs of the same changes taking place in coagula while they are still moist, and also in the blood when it is

extravasated, and coagulates in the interior parts of living animals.

I have therefore reconsidered this subject, and with the assistance of Mr. BAUER, have instituted a series of experiments, and had drawings taken to elucidate their results, which I hope will do away the objections made against coagulated blood having channels formed through it by the extrication of carbonic acid gas; and prove, not only that the same change takes place when blood is extravasated in living animal bodies, but that these channels have a communication opened between them and the neighbouring arteries, and that the fluid blood circulates through the channels in the coagulum. In laying these experiments and drawings before the Society, I request that I may be indulged in adding them to the Croonian Lecture; and with these additions, that it may be submitted to the judgment of the Council.

As the measurement of spherical bodies is a subject on which I am totally unfit to form an opinion, I requested my friend Captain KATER to have the goodness to measure the diameter of a globule of the blood, in what appeared to him the most satisfactory manner, and to explain to me the mode of doing it. He very readily complied with my request, and the following is the mode which he adopted.

A ruler divided into inches and tenths was placed on the box which supports the microscope, a mother of pearl micrometer scale was placed under the microscope, each division of which was equal to one two-hundredth of an inch: viewing ~~the~~ with both eyes open, its image appeared to be projected on the ruler, and one division appeared to subtend the space of one inch. The micrometer scale being removed, blood

sufficiently dilute was placed under the microscope, and being viewed with both eyes open, a globule of blood appeared to occupy, in the first experiment, one half of one tenth of an inch, and in the second experiment, one third of one tenth of an inch upon the ruler. Hence the size of the globule by the first experiment will be equal to

$\frac{1}{2}$ of $\frac{1}{10}$ of $\frac{1}{200}$ of 1 inch = $\frac{1}{4000}$ of an inch;
and by the second experiment

$\frac{1}{3}$ of $\frac{1}{10}$ of $\frac{1}{200}$ of 1 inch = $\frac{1}{6000}$ of an inch;
the mean of which, or $\frac{1}{5000}$ of an inch, may be considered as about the mean diameter of a globule of the blood.

This measurement of Captain KATER's, as it was natural to expect, corresponds with that which has been made by Dr. WOLLASTON, by means of a very ingenious micrometer of his own invention, a description of which has a place in the Philosophical Transactions; and with the measurement of Dr. YOUNG in his eirometer, of which he has given an account in his Introduction to Medical Education.

The diameter of a globule of the blood, measured by mathematicians of such eminence, is to be set down as $\frac{1}{5000}$ part of an inch; and the diameter in the micrometer, measured with all the accuracy that instrument is capable of, since such was the smallest apparent dimension which occurred in Mr. BAUER's experiments, as $\frac{1}{2000}$ part of an inch.

I have taken more pains to have the difference between the measurement of a globule in these different modes ascertained, than the subject would appear to require; but its being known, will enable microscopical observers, unskilled in the higher branches of mathematics, to pursue their observations upon globules of different sizes, and continue to compare their re-

lative size in the micrometer, which will give a correct result which ever mode of measurement is adopted respecting the original globule.

To do away the objection which has been made to gas being contained in the net-work formed in coagulated blood, I first made the following experiment. I placed a vessel nearly filled with blood drawn from the arm, under the receiver of an air pump, and by exhaustion extracted the gas contained in the blood. This blood deprived of its gas when coagulated, exhibited no appearance of net-work. In that part which had coagulated before the exhaustion was completed, the net-work was beautifully distinct.

When blood is drawn from the arm into a cup, and allowed to stand 48 hours, the serum is separated, and every where encloses the coagulum. The greater part of the surface of the coagulum is covered with small round holes in which the gas had collected, and then forced its way out into the serum. But if blood taken by cupping is allowed to stand 48 hours in a cup, sometimes the serum is only separated in small quantity, and does not rise above the coagulum, in consequence of a film or pellicle forming on the surface of the coagulum, and fixing itself to the edge of the cup all round. This pellicle when examined at the end of 48 hours appears to contain ramifying vessels. This arises from the mode by which the blood is extracted depriving it of a part of its carbonic acid gas, and what remains is not sufficient in quantity to burst the pellicle, and when in the act of extrication it arrives immediately under the pellicle, it is forced to spread in different directions, putting on this appearance.

Having ascertained that this appearance is produced by the

extrication of carbonic acid gas, I was led to make the attempt to inject the coagulum with the common minute injection under the receiver of an air pump. The experiment was made in the following manner. A glass cup about an inch and a half deep, and nearly three inches in diameter, had blood from the arm drawn into it, till it was three parts full. The blood was allowed to stand in a cool place for 48 hours, the serum was then strained off, and about $\frac{1}{8}$ of the coagulum on one side was cut away and removed, and the cavity thus made was filled with common red size injection in a fluid state; not, however, quite so high as the surface of the coagulum. In cutting off a portion, the edge showed the coagulum to be very weak, much more so than it is commonly met with. The glass vessel was immediately put under the receiver of the air pump. Very early in the exhaustion, the carbonic acid gas was evolved in such quantity as to keep the fluid injection in a state of agitation, which had the advantage of keeping it fluid; when the exhaustion was increased, the evolvement was so rapid that it became necessary to work the pump very slowly. After the exhaustion had become nearly complete, the glass vessel and its contents were removed, and, with a view to fix and harden the coagulum, the glass vessel was placed in boiling water, which was renewed at short intervals, carefully preventing the water from coming in contact with the blood. This process melted the injection that had not passed into the coagulum, and allowed of its being poured off. The coagulum even now was by no means very firm, but capable of supporting itself; it was turned out upon a flat piece of glass. To make it dry more readily, and to prevent its going into

putrefaction, after having stood six hours, it was divided into slices half an inch thick. Forty-eight hours after it had been injected, I examined it with Mr. BAUER, and found its internal substance very minutely injected, there being only two small bursts of extravasation, each the size of a pea. Mr. BAUER's drawings of the appearance the injection put on immediately under the surface of the coagulum, and upon the cut edge of one of the sections, are annexed, and require little description, so perfectly do they represent the objects from which they were taken. I was assisted in this and the other experiments by Mr. CLIFT and Mr. GARCOMBE. This experiment decides the question respecting the structure of the net-work; since now the channels are filled with injection, their shape, size, and mode of ramification admit of every one examining them for himself; and one of the slices of the coagulum, in which this structure is seen, I have been able to preserve in spirit, and two others in oil of turpentine, so that the originals, as well as the drawings, are brought before the Society.* As the injection could only fill the spaces from which the carbonic acid gas was extracted, it cannot be doubted that the channels were formed by the gas.

Having brought these facts in proof of channels being formed in coagulated blood out of the body, and of their depending upon the evolvment of the carbonic acid gas contained in the blood, it next became necessary to ascertain whether coagula of blood deposited in the abdomen underwent the same change. To determine this point, I wounded one of the smaller branches of the mesenteric artery of a

* These specimens, two of injected coagula of venal blood, and one of arterial blood, are deposited in the Collection of the Royal College of Surgeons in Lincoln's-Inn-Fields.

rabbit, and the wound in the abdomen being closed, this small artery was allowed to bleed into the general cavity. In 48 hours the animal was pithed, and the arteries of the abdominal viscera were injected by the common minute injection coloured by vermillion. The cavity of the belly was then opened; it was in a perfectly natural state; there were no adhesions of parts; the small intestines were very vascular, and the vessels minutely injected. No serum was met with, and no large coagula of blood. One very small one was found lying upon the peritonæum in the right iliac region, and adhering to it in parts, but not by the whole surface of contact. This coagulum was evidently injected, and the only one I particularly examined, as there only remained one or two smaller coagula lying upon a portion of the intestinum ilium. All the rest of the blood that had been effused was absorbed. A drawing taken from this coagulum in the microscope, by Mr. BAUER, is annexed. He has shown the small arteries of the peritonæum entering the channels in the coagulum, which are larger than the arteries with which this communication has been formed; their appearance is also very different from that of arteries; they seem to have been over distended by the injection, and not to have acquired a regular form: there are three or four points of communication laterally between the channels in the coagulum and the arteries of the peritonæum, and it would appear that there is another communication immediately behind the centre of the coagulum. In all of these points, the smallness of the arteries, when compared with the channels themselves, is equally distinct. *Vide Plate XII.*

The appearance this injected coagulum put on, brought to

my recollection a preparation I had made in the year 1788, in which a small pedicle of coagulable lymph adhered to the surface of a portion of intestine, and had become vascular in less than twenty-nine hours, since the person died in that period after the operation for the strangulated hernia, and the intestine, when returned, had the natural polish on its surface. I had succeeded in injecting the arteries leading to it. An account of this case is published in the Appendix to my work upon Ulcers, and a drawing from the preparation is engraved in Mr. HUNTER's work upon the Blood, Inflammation, and Gun-shot Wounds, but from not being magnified, the parts are indistinctly seen.

I requested Mr. BAUER to examine this preparation, which is preserved in the Hunterian Collection of Morbid Anatomy, and to make a magnified drawing of it upon nearly the same scale as that of the coagulum of blood, to show the difference, if there is any, between the appearance of the channels formed in exuded coagulable lymph, and extravasated blood. He has done so; and they correspond in the most essential particular, which is that the canal in the coagulum is larger in diameter than the artery by which it is supplied with blood. His drawing is annexed. Plate XIII.

By this means I have been enabled to present to the Society the appearance these channels put on, both in coagula of blood extravasated in the cavities of living animal bodies, and in exudations of coagulable lymph, at nearly the shortest possible periods in which they can be formed.

There is a preparation in the Hunterian Collection, of a coagulum of blood lying upon the testicle, of considerable size, which was produced by wounding an artery in tapping

a hydrocele ; and from the circumstance of the testicle being extirpated a month afterwards, it was ascertained that the coagulum had remained there four weeks before the extirpation. The parts, immediately after the operation, were injected by Mr. HUNTER, and the coagulum was found to be vascular. This is the most satisfactory proof of coagulated blood having vessels Mr. HUNTER had met with. He has given several engravings of the coagulum, in his work on the Blood ; but, from Mr. BELL not having Mr. BAUER's knowledge of drawing such objects in a magnified state, little information is to be received from an examination of the engravings. It occurred to me, that by a re-examination of this preparation, and putting a thin section of the coagulum, continuing the section into the substance of the testicle on which it lay, into the hands of Mr. BAUER, he might be able to make a magnified drawing of the parts, which would enable me to show, by comparing this drawing with the others, what changes are produced in the appearance of the channels, after they have for so long a time received the circulating blood ; and whether the arteries by which they are supplied, had enlarged sufficiently to convert them into subordinate branches. The passages or channels originally formed by the extrication of the carbonic acid gas, were now found to have acquired a distinct coat, that admitted of being separated from the surrounding parts, showing them to be formed into regular tubes, but they still remained larger than the branches of the arteries by which they were supplied with blood, as will be seen by inspecting Plate XIV.

As the globules of pus are similar to those of the blood, I made experiments upon the fluid in which they are suspended,

and found inspissation produce the same effect on it, as coagulation does on the other; that a similar net-work is formed, and apparently by the same means, since if pus is deprived of its carbonic acid gas (of which it contains a large quantity) by exhaustion in the air pump, no such net-work takes place.

This is a fact of considerable importance in practical surgery, for since we now know that inspissated pus can become vascular, similar to coagulated blood, we have arrived at the principle on which granulations are formed, and from whence they derive the power of contraction, which is found to be inherent in them; we also can account for the great advantage of compression upon the surface of sores; since by that means all the superfluous pus is removed, leaving only enough for inspissation, in which state the carbonic acid gas is extricated, forming channels so as to admit of its becoming afterwards vascular, and then taking on the form of healthy granulations.

EXPLANATION OF THE PLATES.

PLATE VIII.

This plate contains six figures: in three of these the globules of the human blood are shown under different circumstances. In the three others are represented different views of the smallest fibre to which a muscle has been reduced in the field of the microscope.

Fig. 1. Shows 16 globules enveloped in their colouring matter, occupying, upon the micrometer, a superficies of 160,000 part of a square inch.

Fig. 2. Shows that when the colouring matter is removed, 25 globules occupy the same space.

Fig. 3. Shews that the globules, deprived of their colouring matter, and allowed to float in the coloured serum, when once in contact, appear to adhere in a manner not seen to take place when the globules are enveloped in the colouring matter.

These three figures are magnified 400 diameters, or 160,000 superficies.

Fig. 4. Muscular fibres from the thigh of a boiled chicken magnified 400 diameters, or 40,000 superficies.

Fig. 5. One single fibre.

Fig. 6. A fibre to shew a variety in its form.

The two last figures are magnified 400 diameters, or 460,000 superficies.

These fibres in their diameter correspond nearly with the globules of the human blood, deprived of their colouring matter.

PLATE IX.

Represents the appearance which a drop of blood puts on immediately after it has been taken from the arm into a watch glass, and magnified 25 diameters, or 625 superficies.

That this appearance is produced by the carbonic acid gas, which is separated from the blood in the act of coagulation, is proved by no such appearance being met with when that gas has been previously removed by exhausting the blood in an air pump.

PLATE X.

Represents the same appearance magnified 200 diameters or 40,000 superficies, and shows that the globules of the blood take no part in forming it.

PLATE XI.

Represents a portion of coagulated blood injected under the receiver of the air pump, with the common minute injection, coloured by vermilion, by exhausting the coagulum of its carbonic acid gas, and the injection taking its place.

This plate contains two figures ; Fig. 1 shows the course of the injection immediately under the pellicle which forms upon the surface of the coagulum : this is every where horizontal ; for as the carbonic acid gas had no means of escape upwards, it was forced to move in an horizontal direction. Fig. 2 shows the injected part of the coagulum exposed in a section, rather oblique than vertical, in consequence of the coagulum being too soft to admit of the section being more direct. The drawing was made before a pellicle had formed upon the cut surface, which, had it taken place, would have rendered the surface opaque. The course of the injection is seen through the semi-transparent coagulum for some way into its substance. The round points are sections of portions of injection which had followed a horizontal course. The parts are magnified 12 diameters, or 144 superficies.

PLATE XII.

Represents a small coagulum injected with coloured injection, from the arteries of the neighbouring parts. The coagulum was formed in consequence of an hæmorrhage from one of the smaller branches of the mesenteric artery, and was deposited upon the peritonæum, near the right groin, only 48 hours before death. It is worthy of observation, that the arteries through which the injection passed to the coagulum, are much smaller than the channels in the coagulum, and these channels are largest in the middle.

The coagulum extends through the whole breadth of the drawing, and the small arteries from the peritonæum open into its substance in several points. The parts are magnified 35 diameters, or 1225 superficieses.

PLATE XIII.

Represents a portion of human intestine, to which a small exudation of coagulable lymph is attached. This is injected from a small artery on the external surface of the intestine, less in diameter than the canal formed in the coagulum.

The opaque bodies lying upon the vein and close to the pendulous coagulum, are uninjected portions of coagulable lymph.

The parts are magnified 12 diameters, or 144 superficieses.

PLATE XIV.

This plate represents a section of a coagulum of blood that had remained in contact with that portion of the tunica vagi-

nalis which is attached to the body of the testicle, for a month before death, and had been afterwards injected with coloured minute injection from the spermatic artery.

The parts are magnified 35 diameters, or 1225 superficies.

The outline of the section of the body of the testicle on which the coagulum lies, and to which it is attached, is distinctly marked, and the exhalant arteries from the covering formed by the tunica vaginalis, are found to be very small, and in no proportion to any part of the tubes in the coagulum with which they have opened a communication.

The canals in this coagulum have acquired a regular shape, having undergone considerable changes since their first formation, which gives them a much nearer approach in appearance to the ramifications of arteries. They have acquired regular coats distinct from the parts by which they are surrounded.

Fig. 1.

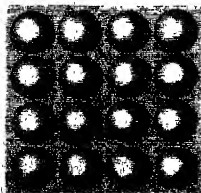


Fig. 2.

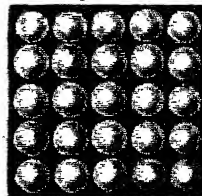
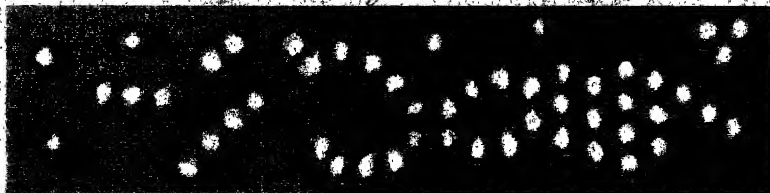
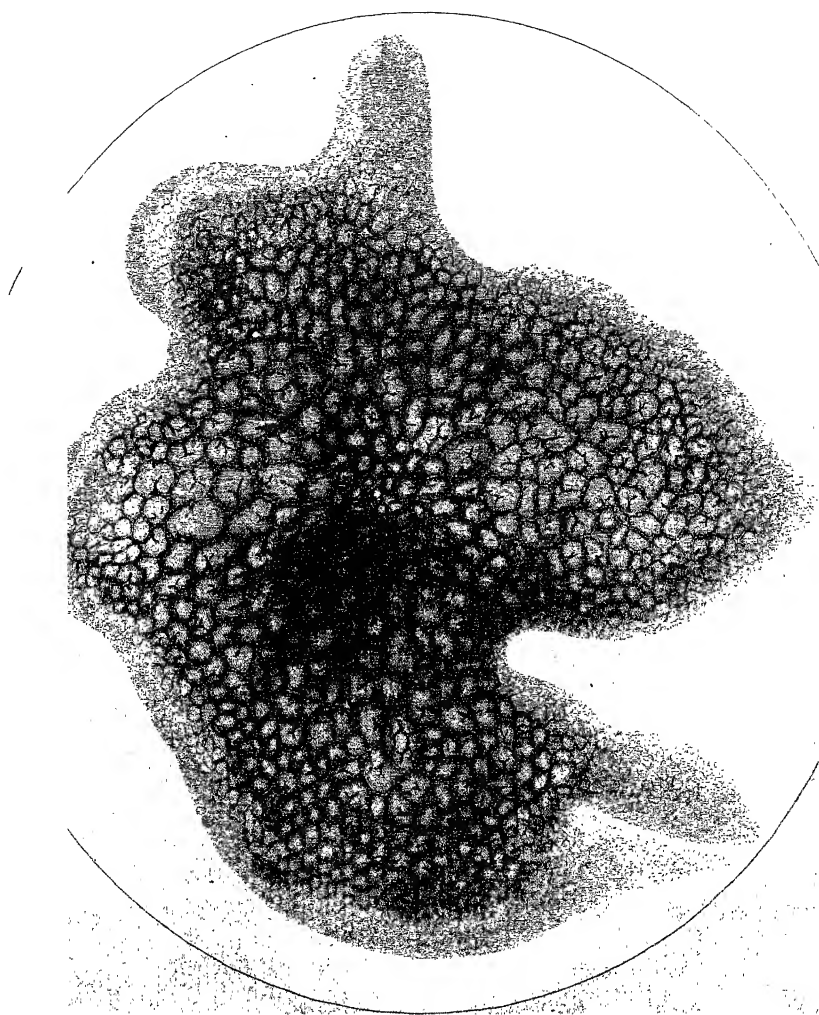


Fig. 3.





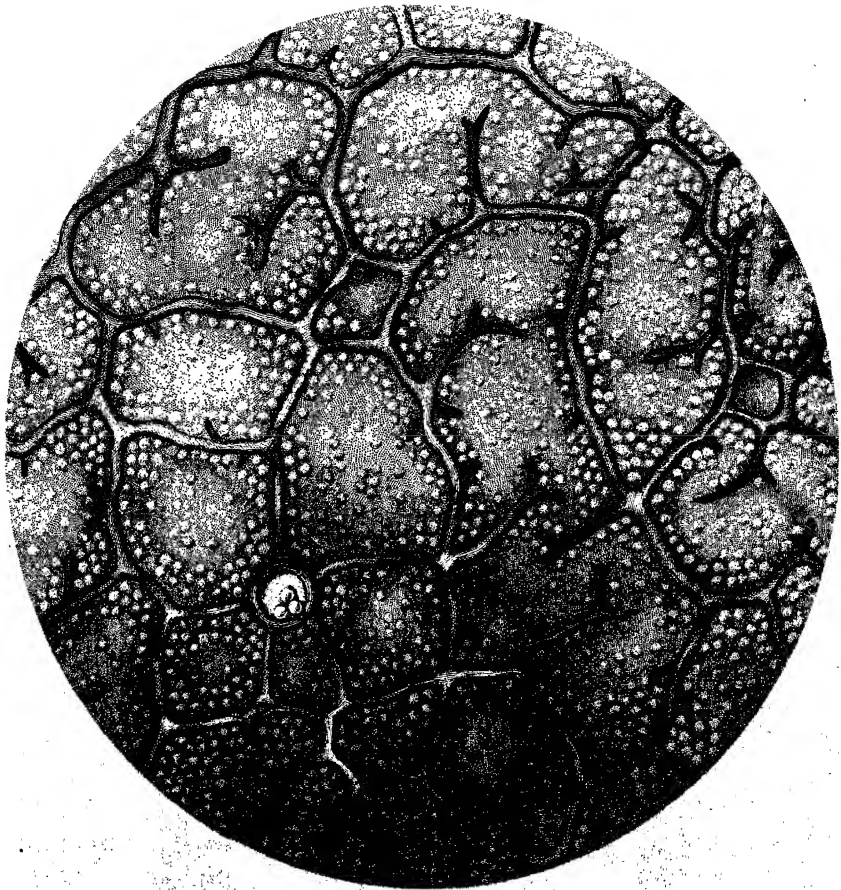


Fig. 1.

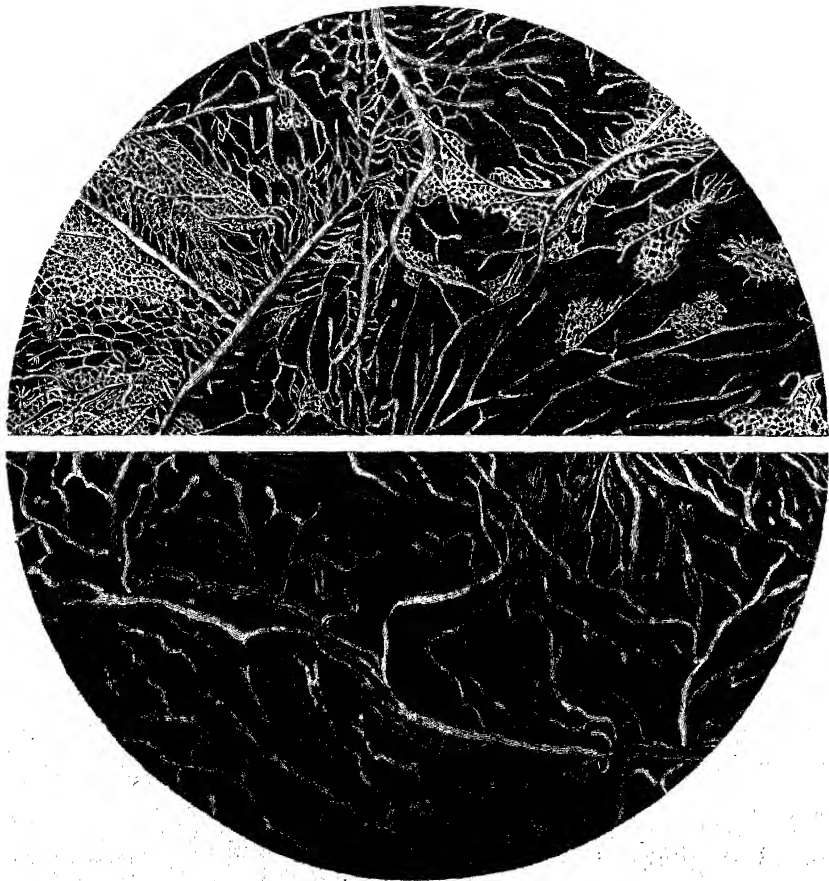
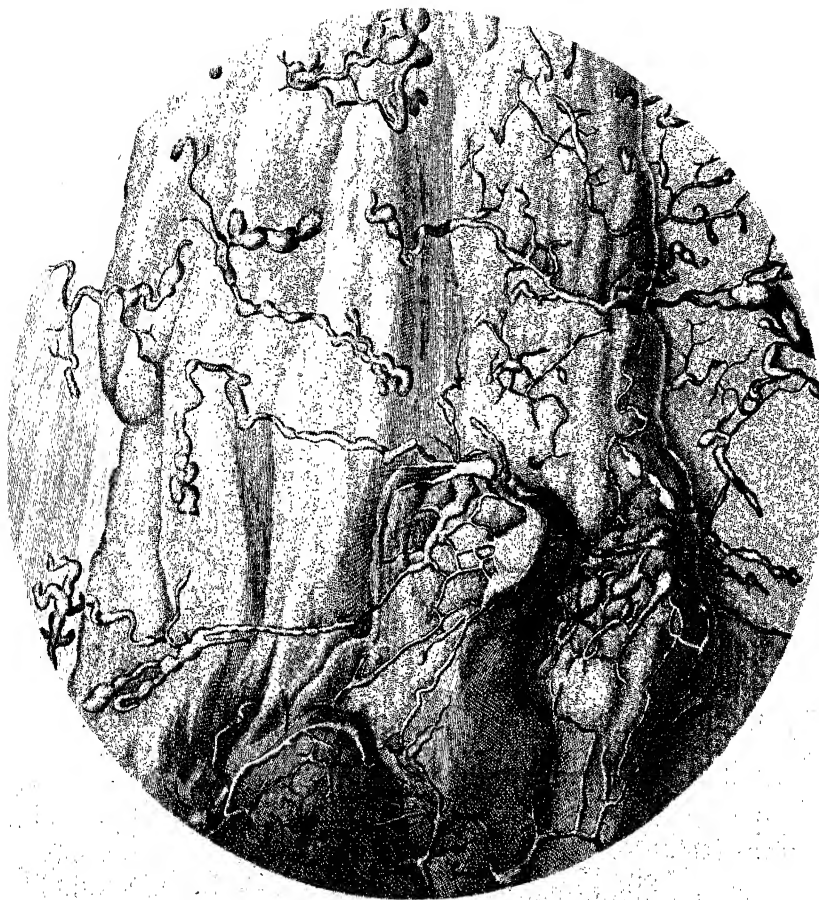
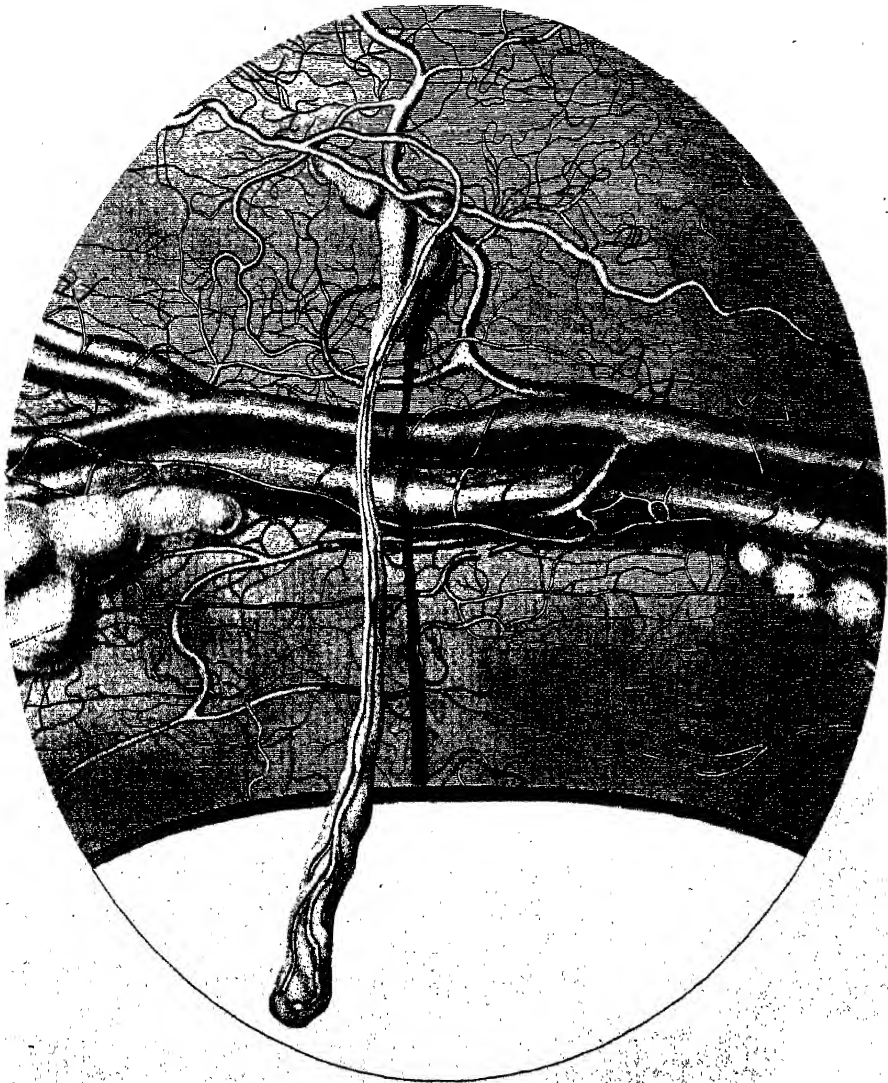
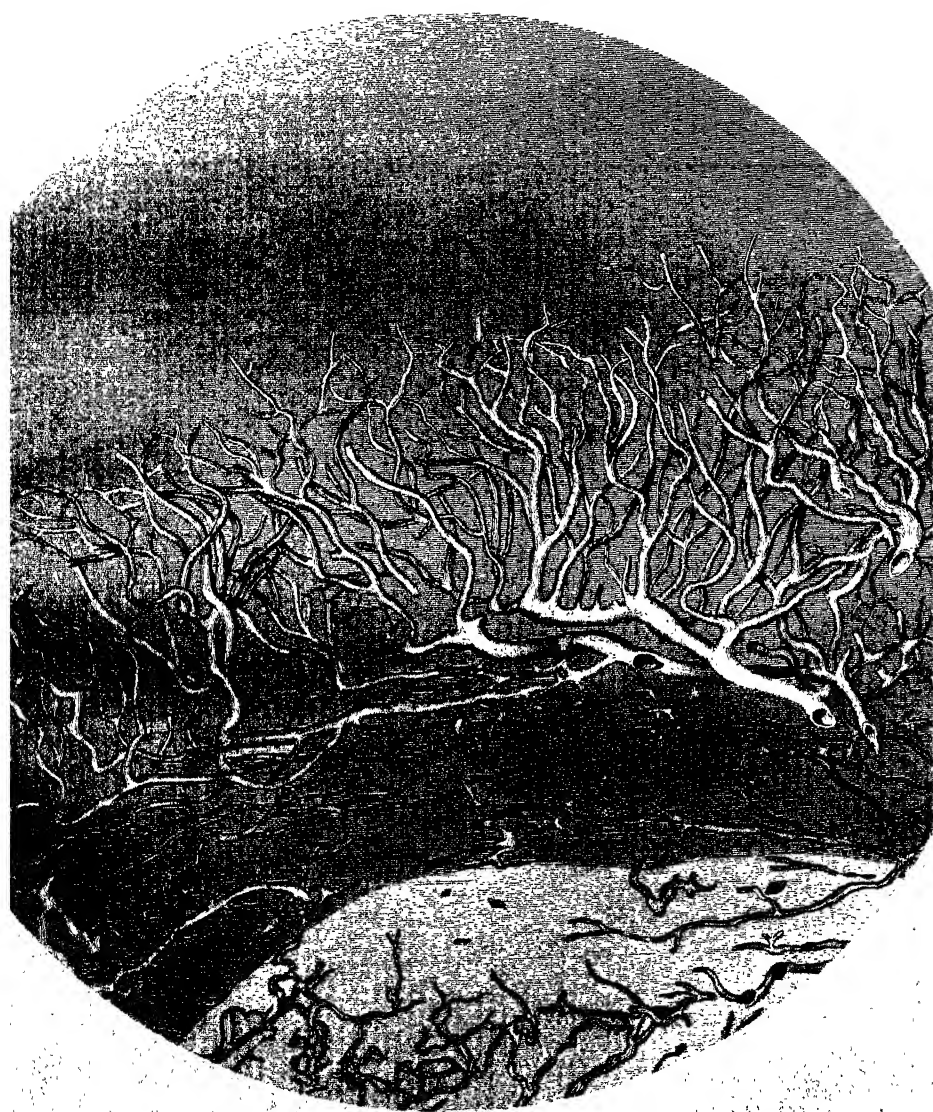


Fig.







XIII. *On the laws of polarisation and double refraction in regularly crystallized bodies.* By David Brewster, LL.D. F. R. S. Lond. and Edin. In a letter to the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.

Read January 15th, 1818.

Venlaw, June 1, 1817.

DEAR SIR,

IN the different memoirs which you have done me the honour of submitting to the Royal Society, I have considered principally those branches of the polarisation of light which relate to the superficial action, or the superinduced properties of uncrystallized bodies. In the course of these enquiries, my attention was frequently directed to the phenomena of regular crystals; but from the difficulty of procuring proper specimens, and the extreme perplexity of the subject, it was not till lately that I succeeded in reducing under a general principle all the complex appearances which result from the combined action of more than one axis of double refraction.

Before I proceed to trace the steps which have conducted me to this general law, I must entreat the indulgence of the Society, while I attempt to give a brief and rapid view of the present state of our knowledge respecting the laws of double refraction. They will thus be able to appreciate more correctly the relative value of those successive generalisations by which this subject has been raised to one of the most interesting departments of physical science.

DR. THOMAS YOUNG was the first person who directed the attention of philosophers to the perfect agreement between the beautiful theory of HUYGENS, and the double refraction of light as exhibited in calcareous spar. (see *Phil. Trans.* 1802, p. 45). The experiments of Dr. WOLLASTON afforded additional evidence of its exactness, and the more numerous and diversified observations of MALUS raised it to the rank of a general principle, which represented all the phenomena in the most accurate manner. About the same time, M. LAPLACE attempted to refer the deviation of the extraordinary ray to the action of those attractive and repulsive forces by which the ordinary refraction and reflection of light are produced. In this theory the aberration of the extraordinary ray is explained by a repulsive force emanating from the short diagonal of the rhomb of calcareous spar, or the axis of extraordinary refraction; and it is shown that the difference of the squares of the velocities of the ordinary and extraordinary ray is proportional to the square of the sine of the angle which this last ray forms with the axis; and that this difference represents that of the action of the crystal upon the two kinds of rays. LAPLACE then demonstrates that the principle of FERMAT, and the principle of least action lead to the law of HUYGENS, provided that in the principle of FERMAT the radius of the ellipsoid is taken to represent the velocity, while in the principle of least action it is made to represent the time in which light describes a certain space taken for unity.

This fine theory will no doubt be received by philosophers as a new proof of the high genius of its author; but while it is

thus received, we ought carefully to consider whether the HUYGENIAN law, upon which it rests, is the universal law of double refraction, or merely an elegant and correct expression of the individual phenomena of calcareous spar. Because this law represents with accuracy the action of a single crystal, is it thence to be presumed that all the other transparent crystals of the mineral kingdom possess the same structure, and exhibit the same series of appearances as calcareous spar? We venture to say that such a conclusion is scarcely possible in the present state of science; and that if it should happen to have been rashly drawn, it could not be deliberately supported by any one who is imbued with the cautious spirit of the inductive philosophy.

It will be replied, however, to this question, that MALUS has examined, with the utmost care, the properties of *quartz*, *arragonite*, and *sulphate of barytes*; that he has demonstrated the identity of their action with that of calcareous spar; and that the extension of the law of HUYGENS to other crystallized bodies can no longer be doubtful. This reply would have possessed considerable weight, had the alledged identity of action been satisfactorily established; but the experiments of MALUS are decidedly erroneous, for out of the *three* crystals, the identity of whose action with that of calcareous spar he is supposed to have demonstrated, no fewer than *two* have more than one axis of double refraction.

Here then is a difficulty of an unexpected kind: for if the experiments of MALUS on these crystals are set aside as incorrect, what confidence can we place in his observations on calcareous spar, upon which the truth of the HUYGENIAN law is founded? If nitre and arragonite, both of which have a

powerful double refraction, had, in place of calcareous spar, been put into the hands of HUYGENS, Dr. WOLLASTON, or MALUS, or of any other philosopher, however skilful, it is demonstrable that their measurements would have accorded with the theory of spheroidal undulations. LAPLACE would have connected this theory with the principles of mechanics, and it would have been universally received as a rigorously physical law. Yet after all this display of experimental and mathematical genius, the whole investigation would have turned out a delusion; for it can be shown, by decisive experiments, that both *nitre* and *arragonite* have two axes of double refraction; and that the aberration of the extraordinary ray cannot be explained by a single ellipsoid.

From these observations, therefore, we conceive it to be proved, not only that the HUYGENIAN law remains undemonstrated as the general law of double refraction; but that it remains undemonstrated as a correct expression of the individual phenomena of calcareous spar.

How then, it will be asked, are we to determine the law of extraordinary refraction, if the best experiments of our most eminent philosophers are set aside as insufficient? This can only be done in two ways, either by the discovery of new methods of magnifying and measuring with great exactness the deviation of the extraordinary ray, when the light passes near the axis of a crystal; or what is more practicable and correct, by examining the appearances which are exhibited by transmitting polarised light along the real or apparent axes of double refraction.

In the year 1811, M. ARAGO discovered, that when polarised light was transmitted through thin plates of sulphate

of lime and mica, and afterwards analysed with a prism of calcareous spar, they exhibited the most beautiful complementary colours. Without knowing what had been done by M. ARAGO, the same discovery was made by me in 1812; but though the general fact which each of us discovered was the same, yet I had the good fortune, by a peculiar mode of observation, to examine the phenomena seen along the real or apparent axes of crystals, and have thus been led to the results contained in the following paper,—results which, as will be seen in the sequel, could not possibly have been obtained by the mode of observation employed by M. ARAGO, and afterwards by M. BIOT.

The last of these philosophers has examined this class of phenomena with singular diligence and ingenuity. By a series of fine experiments, he has established many fundamental points in the science, and has associated his name with this branch of physics, as one of its most distinguished and successful cultivators. *The crystals which M. BIOT examined were

Calcareous spar.	Tourmaline.	Sulphate of lime.
Rock crystal.	Feld-spar.	Sulphate of barytes.
Beryl.	Arragonite.	Sulphate of strontian.
Phosphate of lime.	Topaz.	Mica.

But his experiments for investigating the laws of moveable polarisation were principally made with *sulphate of lime*, on account of its perfect transparency, and the facility with which it can be separated into thin plates. The general result of these experiments is, that all the preceding crystals, with the single exception of certain specimens of mica, have

only one axis of double refraction from which the polarising force emanates; that all crystals are divisible into two classes, namely, attractive and repulsive; and that the laws investigated for sulphate of lime, abstracting the effects of certain secondary forces, are the general laws of polarisation for all other crystals.*

This view of the laws of polarisation, though deduced from ingenious investigations, and by methods which exhibit the finest talents for physical research, is incompatible with the experiments and observations contained in the following paper. No fewer than *seven* out of the *twelve* minerals employed by M. BIOT, have two or more axes of double refraction. Sulphate of lime itself belongs to this number; and all the irregularities of its action, which M. BIOT has represented by empirical formulæ, are the legitimate and calculable results of two rectangular axes. The division of crystals into attractive and repulsive, and the reference of the phenomena of mica to two repulsive axes, whose intensities are as 100 to 677, will also be found to be entirely hypothetical.

From this slight sketch of the present state of the science, it will be readily seen that the process of generalisation, however ingeniously it has been managed, has been carried on too rapidly, and has far outrun the progress of observation and experiment. In attempting, therefore, to establish new and more general laws, I began my researches by laying a broad foundation of facts. One hundred and sixty-five crystals were subjected to examination. In 165 of these, I have observed the property of double refraction. In about 100, I

* See BIOT's *Traité de Physique*, tom. iv. p. 377.

have been able to ascertain whether they have one or more axes; and by examining and measuring the tints which they exhibit at various angular distances from the axes whence the forces emanate, I have been led to a general principle which embraces all the phenomena, and extends to the most complex, as well as to the most simple developement of the polarising forces. This general principle is in no respect an empirical expression of the facts which it represents, nor is it supported by any empirical data. Founded on the principles of mechanics, it is a law rigorously physical, and enables us to calculate all the tints, &c. of the coloured rings, and all the phenomena of double refraction, with as much accuracy as we can compute the motions and positions of the heavenly bodies.

In the course of researches embracing the examination of such a great number of crystals, it was natural to expect that many new facts would present themselves that were but slightly connected with the general object of investigation. The phenomena of this kind, which I have had the good fortune to observe, comprehend several new properties of light, and of crystallized bodies, which I shall lay before the Society in a series of separate communications.

SECTION I. *On the crystals which produce double refraction.*

The property of double refraction was observed by the Abbé HAUY in *twenty* crystallized substances.* MALUS has given a list of *nineteen* doubly refracting crystals, embracing

* *Traité de Mineralogie*, tom. i. p. 271, 272, Paris, 1801.

all the bodies in HAUY's table, except topaz, which he has accidentally omitted.*

The faculty of depolarisation, which I have explained in a former paper,† has been considered as a sufficient indication of the existence of two separate images; and upon this principle it has been stated, that all crystals have the property of double refraction, whose primitive form is neither the cube nor the regular octohedron.‡ This statement, however, is by no means correct, for the rhomboido-dodecahedral crystals ought also to have been excepted; and I have besides shown, by numerous experiments, that almost all the crystals which have these forms, exhibit an imperfect doubly refracting structure, which some of them possess to a very considerable degree. But admitting the statement to be unexceptionable, it could never have been used as a rule for determining whether a crystal refracts doubly or singly; for it is much more difficult to detect the primitive form of a crystal, than to examine, by direct experiments, its optical properties. Tungstate of lime, for example, would have been reckoned a crystal without double refraction, when HAUY believed its primitive form to be the cube, although it possesses this property in a very high degree.

In order, therefore, to determine whether crystals have the property of double or of single refraction, we must ascertain by direct observation, if they form two images, or possess the property of regular depolarisation. In this way I have obtained the results in the following table; but as the exist-

* *Théorie de la Double Refraction*, p. 96, Paris, 1810. † *Phil. Trans.* 1815, p. 27.

‡ *Edin. Transactions*, vol. viii. Part. I. Biot's *Traité de Physique*, tom. iii. p. 325.

ence of two separate images is the only correct and infallible test of double refraction, I have employed it for the greater part of the substances in the table. When a crystal possesses the property of depolarising light, it only proves that it forms two pencils polarised in opposite planes; though there can be little doubt that one of them is subject to the extraordinary law of refraction.

Table of doubly refracting crystals.

Chromate of lead	Tourmaline
Carbonate of lead	Rubellite
Tungstate of lime	25 Quartz
Carbonate of potash	Agate
5 Nitrate of potash	Dichroite
Calcareous spar	Bitter spar
Arragonite	Apatite
Zircon	30 Idocrase
Sulphur	Mica
10 Acetate of lead	Lepidolite
Acetate of nickel	Talc
Ruby	Indurated talc
Sapphire	35 Chlorite
Corundum	Kyanite
15 Beryl	Stilbite
Emerald	Cubizite
Cymophane	Apophyllite
Peridot	40 Prehnite
Epidote	Hydrate of magnesia
20 Euclase	———— barytes
Topaz	———— strontites
Pycnite	Feldspar

45	Mellite	Spodumene
-	Realgar	Tabular spar
	Native orpiment	Benzoic acid
	Ruby silver	Oxalic acid
	Actynolite	80 Citric acid
50	Anhydrite	Tartaric acid
	Litharge	Boracic acid
	Cinnabar	Phosphoric acid
	Tincal, or native borax	Succinic acid
	Borate of soda	85 Chromic acid
55	Mesotype	Cryolite
	Sphene	Benzoate of ammonia
	Harmotome	Sulphate of cobalt
	Macle	————— lead
	Nepheline	90 ————— iron
60	Augite	————— zinc
	Datolit	————— copper
	Wavellite	————— copper and iron
	Calamine	————— nickel
	Anthophyllite	95 ————— barytes
65	Laumonite	————— strontites
	Titanite	————— lime
	Asbestos	————— magnesia
	Serpentine	————— potash
	Steatite	100 ————— soda
70	Diopside	————— ammonia
	Staurotide	————— magnesia and
	Diallage	soda
	Hyacinth	Carbonate of copper
	Tremolite	————— barytes
75	Semiopal	105 ————— strontites

Carbonate of soda		Phosphate of iron	
_____ ammonia	135	_____ copper	
_____ copper		_____ lead	
Muriate of mercury		_____ soda	
110 _____ gold		_____ sub of potash	
_____ silver		Arseniate of lead	
_____ copper	140	_____ iron	
_____ iron		_____ copper	
_____ lead		_____ potash	
115 _____ magnesia		Tartrate of potash	
_____ lime		_____ potash and soda	
_____ barytes	145	_____ super of potash	
_____ strontites		_____ potash and an-	
Nitrate of silver		timony	
120 _____ copper		Oxalate of ammonia	
_____ zinc		Superoxalate of potash	
_____ bismuth		Prussiate of potash	
_____ mercury	150	Hyperoxymuriate of	
_____ ammonia		potash	
125 _____ lime		Oxymuriate of mercury	
_____ strontites		Calomel	
_____ soda		Mother of pearl	
_____ magnesia and		Ice	
ammonia	155	Camphor	
Acetate super of copper		Sugar	
130 _____ super of copper		Crystallized Cheltenham	
and lime		salts	
_____ zinc		Murio-sulphate of iron	
_____ soda		and magnesia	
_____ potash		Emerald copper	

160	Muriate of lead	manganese
	Sulphate of ammonia and	———— alumine
	magnesia	Specular iron
	———— red oxide of	165 Pargasite

SECTION II. *On crystals with one apparent axis of polarisation.*

Having thus distinguished the crystals which possess double refraction, from those which are destitute of this property, we shall now proceed to determine their optical structure, or to ascertain whether the forces which act upon the extraordinary ray emanate from one or more axes.

If we transmit polarised light through any of the parallel surfaces of the primitive hexaedral prism of a crystal of beryl, and analyse the emergent pencil by a prism of calcareous spar, having its principal section placed either parallel or perpendicular to the plane of primitive polarisation, it will be found that, when the axes of the prism of beryl is inclined 45° to the plane of primitive polarisation, the vanished image is restored. If the thickness of the crystal exceeds 0.035 of an inch, the restored light will be nearly white.

But if the polarised light is transmitted along the axis of the prism of beryl, there will be seen a series of beautiful circular concentric rings whose centre is the intersection of the arms of a dark rectangular cross, and containing all the tints in NEWTON'S table of the colours of thin plates. These rings, which are shown in Plate xv. fig. 1, increase in diameter as the length of the prism is diminished; but they may be distinctly seen, even in the longest prism of beryl. If we now examine the double refraction of beryl by transmitting

force, by the action of which the coloured rings are produced; but, in all those crystals upon which this experiment can be made, I have invariably found that the two axes from which these forces emanate, are coincident, and that the force which produces the deviation of the extraordinary ray increases and diminishes with the polarising force which produces the coloured rings.*

In proceeding to examine the nature and properties of the coloured rings produced by the different crystals in the table, we shall first consider the effect produced by plates of different thicknesses. If we take a rhomboid, of calcareous spar, whose principal section is $A B C D$, fig. 2. and cement upon its two surfaces AB, CD , two prisms of flint glass, having their sides perpendicular to the principal section, and their refracting angles EBF, GDH , a little greater than FBb which is $45^{\circ} 29' 26''$; and if polarised light is incident, perpendicularly upon the surfaces BE, DG , it will be transmitted parallel to Bb ,† and will exhibit in the most beautiful manner the system of coloured rings shown in

* M. BIOT was the first who deduced this conclusion for crystals with one axis from experiments on calcareous spar and rock crystal; but it could not be considered as a general fact when drawn from such a small number of crystals. It will be seen in the sequel of this paper, that I have extended the result to crystals with two axes, and have thus established it as a general principle.

† This mode of exhibiting the coloured rings, is greatly superior to the mode employed by BIOT, of grinding down the solid angles at B and D . Beside the advantage of procuring the greatest possible thickness from a given rhomboid, we preserve the polish of its natural faces, and the thickness of crystal through which the polarised ray passes can be calculated with the utmost accuracy from the thickness Bb of the plate. This method is also peculiarly favourable for showing the influence of pressure upon the polarising structure of the crystal, and for various other experiments on the coloured rings.

fig. 1.* Let the rhomboid ABCD be now cut into two plates, by any line MN, and let the rings, produced by each of the plates, be examined separately in the way already described: it will then be found that the squares of the diameters of the rings are in every case proportional to the numbers which represent the corresponding tints in NEWTON's table; and that the squares of the diameters of similar rings, as produced by plates of different thicknesses, are reciprocally proportional to the square roots of these thicknesses. These two results, which were first obtained by M. BIOT, lead to the general conclusion that the tints produced at different inclinations to the axis of the crystal, are to one another, as the square of the sine of the angle which the polarised ray forms with that axis. This law of the tints for crystals with one axis, was deduced by M. BIOT from the action of rock crystal, as well as calcareous spar; and also from the phenomena of certain specimens of mica which he supposes to have one axis. I have found it perfectly correct in all the other crystals contained in the table; and it may therefore be considered as a general law, which we may apply with confidence in our future researches.

* The system of coloured rings produced by one axis of double refraction, and the still more beautiful and complicated system produced by two axes, were discovered by me in the year 1813. I observed the former in *beryl, emerald, ruby, &c.* and the latter in *topaz, mica*, and a great variety of other minerals. Dr. WOLLASTON was the first who detected the circular system of rings in *Iceland spar*, and they were shown to me by that eminent philosopher in July 1814. In a letter dated Dec. 3, 1815, M. BIOT announced to me, that he had then discovered the circular rings in *Iceland spar*, and it appears that the same observation was made by M. SEEBECK, in Dec. 1815; these dates, however, are nearly a year and a half posterior to that of Dr. WOLLASTON's experiment.

In order to apply it however with the utmost simplicity, I have found it very convenient to consider every crystal as cut into a sphere, one of whose diameters is the axis of double refraction, and to suppose that the polarised ray passes through the centre of the sphere. By this means we have no occasion to consider the refractive power of the crystal, or the difference of thickness arising from oblique transmission; for the polarised ray is always incident perpendicularly, and the thickness of the sphere is every where the same. If in this sphere, AB, fig. 3. is the axis of the system of rings, then Pp may be called the *diameter of no polarisation*, or the *apparent axis of double refraction and polarisation*; P and p the *poles of no polarisation*; COD, the *equator of maximum polarisation*, and EF the *isochromatic lines or curves of equal tint*. Now, since the tint varies as the square of the sine of the angle which the transmitted ray forms with the axis Pp, it will be a *maximum* in every point of the equator COD, and will be represented by $\text{Sin}^2. 90^\circ$. Hence if ϕ be the angle which any other diameter EO forms with the axis, the tint at E, and at every point of the parallel EF, will be represented by $\text{Sin}^2 \phi$. By determining therefore experimentally the tint t , produced at any given thickness B, and at any inclination ϕ , the maximum tint T, for that thickness, will be $T = \frac{t}{\text{Sin}^2 \phi}$ and the tint for any other thickness b will be $T = \frac{b}{B} \times \frac{t}{\text{Sin}^2 \phi}$. It is obvious from these formulæ, that any given tint can be developed at any angle or distance from the axis, merely by varying the thickness of the crystal or the diameter of the sphere.

If it should be required to find the tints corresponding to any angle of incidence upon the natural faces of the crystal,

we have only to consider the position of our sphere within the crystal, and compute the inclination of the refracted ray to the axis. Thus in *calcareous spar* the axis *Pp* corresponds with the short diagonal *AB* of the primitive rhomb, as shown in fig. 3. In *beryl*, *zircon*, *sapphire*, *rubellite*, *rock crystal*, *nepheline*, *idocrase*, *muriate of lime*, *super acetate of copper and lime*, *muriate of strontian*, &c. it corresponds with the axis of the prism in which they generally crystallize; while in *hydrate of magnesia*, *arseniate of copper*, &c. it is perpendicular to the laminæ of which they are composed.

Although the systems of rings produced by all crystals with one axis, exhibit the same tints, and possess the same properties, yet those which are produced by the five crystals of the negative class, namely, *zircon*, *quartz*, *ice*, *super acetate of copper and lime*, and certain specimens of *sulphate of potash*, will be found to differ in one essential particular from the system produced by all the other crystals. If we take two rhomboids of *calcareous spar* and place the one symmetrically on the other, the system of rings which they exhibit will be exactly the same as would have been produced by one rhomboid whose thickness is equal to the sum of the thicknesses of the two which are combined. M. BIOT has deduced this result from theory, and attempted in vain to confirm it by experiment;* but from the peculiarity of the method of observing

* J'ai employé ainsi conjointement les deux plaques de spath d'Islande dont j'ai donné plus haut les épaisseurs; et j'ai eu en effet des anneaux bien plus petits que par une seule d'entre elles; mais la difficulté d'aligner également les axes de ces deux plaques m'a empêché de mettre dans les expériences l'exactitude nécessaire pour les mesurer. J'aurais voulu aussi combiner des cristaux attractifs avec des cristaux repulsifs, mais je n'en ai pas eu l'occasion. BIOT *Traité de Physique*, tom. iv.

the rings shown in fig. 2, I have been able to prove it by direct observation. The exact parallelism of the sides of the two rhomboids, when their surfaces are not perfectly flat, can easily be obtained by separating them with a piece of soft wax, and observing the perfect coincidence of the reflected images from the two adjacent surfaces; or by looking at the system of rings, and altering the position of the crystals till they become quite perfect. I have obtained a similar result by combining a plate of *beryl* with a plate of *calcareous spar*, for the system of rings will always be the same as would have been produced by two plates of *beryl*, one of which was the plate employed, and the other a plate which gave rings of the same size as the plate of *calcareous spar*.

But when we combine the system of rings produced by a crystal of *zircon*, &c. with the system produced by *calcareous spar*, a very different effect is produced. The system of rings instead of being diminished is increased, and is equal to the system which would have been produced by a thin plate of *calcareous spar*, whose thickness is equal to the difference of the thicknesses of the plate of *calcareous spar* employed, and another plate of *calcareous spar*, that would give rings of the same size as those given by the *zircon*, &c. alone. This result, which I succeeded after much labour in obtaining experimentally, will also be obtained by substituting ice in place of *zircon*. Quartz cannot be employed in these experiments, as the system of rings is never complete, on account of the secondary tints which M. Biot discovered along its axis. If the plate of *zircon*, &c. gives a system of rings of the very same size as those of *calcareous spar*, the one system will be com-

pletely obliterated by the other, and the combined crystals would exhibit neither double refraction nor polarisation. Hence it follows that *zircon, quartz, and ice, superacetate of copper and lime*, and certain specimens of *sulphate of potash*, form a class separate from all the other crystals in our table ; and that the polarising force in the one class, is either of an opposite nature, or exerted in an opposite direction to the polarising force in the other class. This opposition in the action of crystals was first observed by M. Bior, in plates cut obliquely to the axis ; and by this mode of observation he divided the crystals which he examined, into two classes, in the following manner :

REPULSIVE CLASS.	ATTRACTIVE CLASS.
Calcareous spar	Rock crystal
<i>Arragonite</i>	<i>Sulphate of lime</i>
Beryl	<i>Sulphate of barytes</i>
Tourmaline	<i>Topaz</i>

But since the crystals marked in italics have two or more axes of double refraction, and cannot, as we shall afterwards show, be ranked in either class, M. Bior's list, when corrected, will stand thus :

REPULSIVE CLASS.	ATTRACTIVE CLASS.
Calcareous spar	Rock crystal
Beryl	
Tourmaline	

agreeing, in so far as it goes, with the more extended table which we have already given. Hence it is obvious that M. Bior could not have made the experiment which he proposes to make, at the end of the passage just quoted ; namely, to

combine the system of rings formed by the attractive crystals, with the system formed by the repulsive ones; for rock crystal being the only attractive substance of one axis with which he was acquainted, was quite unfit for this purpose, from the imperfection of the system of rings which it produces. The discovery of the optical structure of *zircon** has enabled me to perform the experiment with success.

This opposition of structure between *quartz* and the other four crystals with one axis, contained in M. BIOT's table, is ascribed by this eminent philosopher to an opposition in the forces which act upon the extraordinary ray; and he considers it as an established point, that in rock crystal, the extraordinary ray is attracted to the axis, while in the other crystals it is repelled from it. Hence he has concluded, that the phenomena may be represented in the first case by a prolate ellipsoid, and in the second case by an oblate ellipsoid as HUYGENS had already shown; and has thus been led to divide crystals into two great classes, *attractive* and *repulsive*. This division, however, is entirely hypothetical, in so far as the polarising forces is concerned, for it will afterwards be shown that the phenomena of rock crystal may be explained by a negative force emanating from two equal rectangular axes, or that the phenomena of the other class may be explained by a positive force in a similar manner; or if we prefer the analogies which the sciences of magnetism and

* The extreme difficulty which attends experiments of this kind, will be understood from the fact, that I cut more than fifteen plates out of a large piece of *zircon* without discovering its axis. By a singular accident, however, Mr. MORRIS, jeweller, in Edinburgh, procured for me no fewer than sixty plates of *zircon* with parallel faces, and it was only in two of these that the system of rings was developed.

electricity present, we may explain the phenomena of both classes, by supposing each crystal to be endowed both with positive and negative axes.

In marking, therefore, this difference of action, I have employed the terms *positive* and *negative*, as denoting merely the *opposition*, and not the *nature* of the polarising forces.

Hitherto we have considered the system of coloured rings as produced only near the axes of crystals, or as capable of being developed at any distance from the axis, merely by diminishing the size of the sphere; but there are two modes by which the rings can be rendered visible at any distance from the axis, and with any thickness of crystals.

If we take a prism of flint glass, with a large refracting angle, and examine through it the system of rings, we shall find that instead of nine or ten rings, which are visible without the prism, we may reckon, by estimation, from 80 to 100 on that side of the axis towards which the refraction is made. This observation is analogous to that of NEWTON upon the rings formed by thin plates, and decidedly proves that the coloured tints are actually produced at distances from the axis where the phenomena of fixed polarisation are exhibited.

The other mode of developing the rings at any distance from the axis, and with any thickness of crystal, consists in crossing the tints with plates of *rock crystal* cut parallel to its axis of double refraction, or with laminæ of *sulphate of lime*. If the axis AB, Pl. XV. fig. 1, of a plate of rock crystal, having such a thickness as to produce no coloured tints, even at a considerable obliquity, is placed in the direction of a diameter of the rings given by calcareous spar, a new system of rings will be produced at A and B, having their centres at O. The

point where A,B, crosses the middle ring will be black, as the action of the calcareous spar is there exactly counter-balanced by the action of the rock crystal. The rings within CD, EF will be *positive*, in consequence of the action of the rock crystal predominating, and those without them will be *negative*, in consequence of the action of the calcareous spar predominating. If the plate of *rock crystal* or *sulphate of lime* produces the rings at *a, b*, instead of A,B, then in the other two quadrants, *c, d*, the tints will be those which are produced by the sum of the actions of the crystals, and the tints at *c* and *d* will be double of that which is produced at these points by the calcareous spar alone.*

One of the most interesting features of the system of coloured rings, is the black cross MN, PR, where the two arms are exactly at right angles to each other. If the plate which produces this system of rings is turned round its axis O, the rings and the black cross will preserve an invariable

* The developement of colours by the opposite action of two crystallized plates, which are not capable of producing them separately, was first effected by M. BIOT, before the discovery of the coloured rings, and by the aid of his divided apparatus ; but the preceding method of developing the rings by these opposite actions, is more general, and is so extremely simple, that it can be easily done by any person, merely by holding the crystals in his hands. The difficulties experienced by M. BIOT are thus stated by himself : " Mais telle est l'exactitude qu'il faut mettre à ces recherches que mes premières tentatives n'eurent aucun succès, parce que je m'étais borné à opérer le croisement à la main au lieu d'employer les appareils divisés qui m'avaient servi jusqu'alors. Car la grande énergie du spath d'Islande fait que si l'on s'écarte le moins du monde des positions indiquées par la théorie, on passe à la polarisation totale et les couleurs ne peuvent plus se développer. Cette cause faisait aussi qu'il était plus difficile de tomber précisément sur la proportion des épaisseurs de chaux sulfatée qui pourrait servir à compenser les lames de cristal d'Islande soumises à l'expérience." *Mém. de l'Institut*. LII 27 Dec. 1813.

position, one of the arms, MN, being always in the plane of primitive polarisation, and the other, PR, in a plane perpendicular to it. Hence it follows, that whenever a plane passing through the axis of the crystal, is either in the plane of primitive polarisation, or perpendicular to it, the extraordinary pencil or tint disappears. This result will be found correct, whatever be the direction in which the polarised ray traverses the crystal, and is therefore the general law of the disappearance of the extraordinary pencil for crystals with one axis.

SECTION III. *On crystals with two or more axes of polarisation.*

The complicated structure of mica was first discovered in 1812 by M. BIOT and myself, without any knowledge of each others observations. M. BIOT considered one kind of mica as the only mineral known to possess this compound structure, indicating the existence of two axes ; whereas, so early as the beginning of 1813, I had found the same structure in *topaz*,* *nitre*, *sulphate of potash*, *tartrate of potash and soda*, *acetate of lead*, and *mother of pearl*; and I have since discovered that this is the general structure of doubly refracting crystals.

The following is a list of crystals, which I have found to possess more than one axis of polarisation and double refraction.

Arragonite	5 Stilbite
Mica	Cymophane
Topaz	Axinite
Feldspar	Olivine

* The drawings of the rings in *topaz*, which I published in the Phil. Trans. for 1814, are an accurate representation of the phenomena, very near the resultant axes of crystals with more than one axis.

	Epidote		Nitrate of copper
10	Kyanite		Borax of the shops
	Talc		Tartaric acid
	Prehnite		Citric acid
	Dichroite	50	Hyper-oxymuriate of potash
	Lepidolite		Phosphate of soda
15	Mother of pearl		Prussiate of potash
	Indurated talc		Tartrate of potash and soda
	Sulphate of lime		Oxalate of ammonia
	———— barytes	55	Super-oxalate of potash
	———— strontian		Crystallized Cheltenham salts
20	———— lead		Rhomboidal salt
	———— magnesia		Murio-sulphate of magnesia and iron
	———— iron		Benzoate of ammonia
	———— copper	60	Mesotype
	———— copper and iron		Oxalic acid
25	———— ammonia		Chromic acid
	———— potash		Muriate of barytes
	———— soda		———— mercury
	———— zinc	65	———— magnesia
	———— cobalt		———— copper
30	———— ammonia and magnesia		Sulphur
	———— soda and magnesia		Hydrate of barytes
	———— nickel		Super-tartrate of potash
	———— manganese	70	Tartrate of potash
	Carbonate of lead		———— potash and antimony
35	———— soda		Boracic acid
	———— ammonia		Succinic acid
	———— potash		Chromate of lead
	Nitrate of silver	75	Diallage
	———— ammonia		Spodumene
40	———— potash		Tincal or native borax
	———— lime		Anhydrite
	———— bismuth		Sugar
	———— mercury	80	Acetate of lead
	———— zinc		———— copper
45	———— strontian		

By examining the preceding list, and comparing it with the list of crystals that have one axis, we may conclude,

1st, that the combinations of sulphuric and tartaric acid with earthy, alkaline, and metallic bases, have two or more axes of double refraction.

2nd, that as only about 23 crystals have one apparent axis of double refraction, while more than 80 have two axes, and as the phenomena of these 23 crystals can be referred to two axes, as will afterwards be shown, the general laws of polarisation and double refraction remain yet to be investigated by experiment.

If we compare the minerals in the preceding list with the most recent table of primitive forms,* it will be found that there is a connection by no means equivocal between the primitive form of a crystal and the number of its axes of extraordinary refraction. In order to establish this curious result, I have drawn up the following Table, in which I have inserted the names of the minerals and the number of their axes opposite to the different primitive forms under which they have been classed.

THREE AXES.	Muriate of soda	} Cube.		Two AXES.	Muriate of barytes	} Right qua- drangular prism with a square base.
	Boracite				Sulphate of magnesia	
	Leucite				Prussiate of potash	
	Analcime				Mesotype	
	Aplome				Sulphate of nickel	
	Phosphate of manganese and iron				zinc	

* The table of primitive forms which I have used, is the one drawn up by Dr. THOMSON in the Article *Crystallography*, in the Edinburgh Encyclopædia, as it contains many of the observations made by HAUY and BOURNON, since the publication of HAUY's *Traité de Mineralogie*.

Two AXES.	Anhydrite Cymophane Prehnite Peridot Stilbite Tartrate of potash	Right qua- drangular prism with a rectangular base.	Two AXES.	Sulphate of iron	Rhomboid with an acute summit.	
	Sulphate of strontian Sulphate of barytes Mica Talc Spodumene Sulphate of soda Carbonate of potash Tartrate of potash and soda Citric acid					
Two AXES.	Sulphate of lime Epidote Axinite	Right qua- drangular prism; base an ob- lique paral- lelogram.	ONE AXIS.	Arseniate of copper Apatite Beryl Emerald Nepheline Sapphire Ruby	Regular hex- aedral prism	
	Borax					
Two AXES.	Oxalic acid Sugar Sulphur Acetate of copper Tartaric acid Chromate of lead	Oblique qua- drangular prism; base a rhomb.	THREE AXES.	Garnet Blende	Rhomboidal dodecahe- dron.	
	Feldspar Kyanite Sulphate of copper					
Two AXES.	Calcareous spar Bitter spar Quartz Tourmaline	Rhomboid with an ob- tuse summit.	ONE & TWO AXES.	Sulphate of potash Fluor spar Muriate of ammonia Alum Spinelle ruby Pleonaste Diamond Ruby copper Nitrate of lead	Bipyrami- dal dode- cahedron.	
	Nitrate of potash Sulphate of soda Arragonite* Topaz Carbonate of lead† Sulphate of lead					
ONE AXIS.		OCTOEDRON IN WHICH THE PYRAMIDS HAVE A REC- TANGULAR BASE.	TWO AXES.	Zircon Mellite Idocrase Apophyllite Arseniate of potash	OCTOEDRON IN WHICH THE PYRAMIDS HAVE A SQUARE BASE.	
TWO AXES.	Carbonate of soda	OCTOEDRON IN WHICH THE PYRAMIDS HAVE A RHOMB FOR THEIR BASE.	TWO AXES.			

* BREWSTER makes it a quadrangular rectangular prism with a square base.

† BREWSTER makes it a quadrangular prism with a rhomboidal base.

It appears from the slightest examination of the preceding Table, that all the crystals with *one, two, or three* axes, range themselves under their particular primitive forms, and that the only exceptions to this general fact, are *Idocrase* and *Apophyllite*, which we have no doubt will be found to have primitive forms, different from those assigned to them by HAÜY. All the crystals with one axis have for their primitive forms a hexaedral prism, a rhomboid with an obtuse summit, and an octohedron in which the pyramids have a square base. All the crystals with three axes, have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive forms; and all the crystals with two axes, crystallize in the various other forms given in the Table.

Hence we are furnished with the means of deducing the number of axes in crystals from their primitive form, and of approximating to the primitive form when the number of axes is given, by excluding certain other primitive forms which belong to a different number of axes. If HAÜY and BOURNON have given these forms correctly for the following substances, in which I have not detected the number of the axes, they will be arranged as in the annexed Table.

ONE AXIS.	TWO AXES.
<p>Tungstate of lime. Octohedrite. Ruby silver. Cinnabar. 5 Carbonate of strontian. Diopase. Harmotome. Chabasie. Cryolite. 10 Phosphate of lead. Glacial sulphuric acid. Carbonate of barytes. Pinite. Spinellane.</p>	<p>Euclase. Calamine. Corundum. Staurotide. 5 Datolite. Sphene. Titanite. Wernerite. Meionite. 10 Paranthine. Acetate of barytes. Calomel. Arseniate of potash. Green carbonate of copper.</p>

1. On the form of the rings or isochromatic curves, and on the nature of the tints in crystals with more than one axis.

We have already seen that when a crystal has only one apparent axis of double refraction, the isochromatic curves, or lines of equal tint are perfect circles having the axis of extraordinary refraction passing through their centres; but when these curves are the result of two separate axes they assume the more complicated form represented in Pl. XV. fig. 4.

If we transmit polarised light in every possible direction through a crystal, COD *o*, Pl. XV. fig. 5, (which is a section of the sphere in Fig. 4, through the great circle COD_o,) we shall find that there are two diameters P *p*, P' *p'* in which there is neither polarisation nor double refraction. To these lines I have given the name of *resultant axes or diameters of no polarisation*, P, P', *p*, *p'* Pl. XV. figs. 4 and 5, being the poles of no polarisation. Each of these poles is surrounded with similar sets of rings, the tints of which commence at P, P', *p*, *p'* increase towards O, C, D, A, and B, and reach their maximum at A and B. When the distance PP' or the inclination of the diameters of no polarisation is considerable, the rings near P, P' are almost circular, but the circularity soon ceases as they recede from the pole. If PP' is less than 90°, the tints at C, D, which are always equal, are higher than those at O, and when P, P' is exactly 90°, the tints at C, O, *o*, D are equal, and the rings are symmetrical round P, P'.

As PP' is always less than 90°, the great circle ACBD may be called the *Equator of maximum double refraction or polarisation*, since the double refraction and the polarisation are always greatest in this line. The great circle AOB_o,

Pl. XV. figs. 4 and 5, may be called the *meridian of direct polarisation*, because the tints ascend directly from O, o to A and B, and the great circle COD o the *meridian of inverse polarisation*, not only because the tints descend from O to P and P', and ascend from P to C and from P to D, but because there is a real inversion in the character of the tints on each side of PP'.*

In *mica*, *topaz*, and other crystals, where the distance PP' is 45° and upwards, the system of rings represented in Pl. XV. fig. 4, cannot easily be seen at once; but in *nitre* and other crystals where PP' is very small, the whole system is finely developed, and every individual curve may be examined with attention.

In order to observe these rings to advantage, let a plate of nitre ACBD, Pl. XV. fig. 6, about $\frac{1}{12}$ or $\frac{1}{15}$ of an inch thick, be cut perpendicular to the axis of the hexaedral prism. If this plate is exposed to polarised light, so that either AB or CD is in the plane of primitive polarisation; and if the transmitted light is analysed with an achromatic prism of calcareous spar, the extraordinary image will exhibit the system of rings shown in Pl. XVI. fig. 7, while the ordinary one will exhibit a system exactly complementary.

The poles P, P' of no polarisation, are distant $5^\circ 20'$, whatever be the thickness of the plate; and through them passes one of the branches CD of the rectangular cross A, B, C, D. The breadth of each ring is least between C and P and D

* The preceding names given to the three great circles, have been drawn from the most prominent physical characters which belong to them. Other names, such as the *meridian of the principal axis*, the *meridian of the secondary axis*, &c. would have been preferable, had they not involved an hypothesis; for it will afterwards be seen that the phenomena may be equally well explained by axes varying both in number and position.

and P, and gradually increases towards AB, owing partly to the different thicknesses at which the light is transmitted through the plate.

When the plate of nitre is turned round its axis, the black cross ABCD immediately breaks up, according to laws which will afterwards be explained; and when AB forms an angle of 45° with the plane of primitive polarisation, the system of rings has the appearance shown in Pl. XVI. fig. 8. The rings themselves have suffered no change by this change of position in the plate; but the black cross is separated into two hyperbolic branches M'P'N', MPN. When the thickness of the plate is above $\frac{3}{10}$ ths of an inch, the portions of the rings included between the two hyperbolic branches, are confounded into one mass of white light, while those between C and P, or D and P, are distinctly visible. The concave sides of the hyperbolic branches are strongly fringed with a red and yellow colour, while the convex or inner sides are equally affected with blue rays. By diminishing the thickness of the plate, the rays gradually appear between the hyperbolic branches, but they consist only of pink and green tints, like those in the 5th order of NEWTON's scale; and even when the rings almost cease to appear by a great diminution of thicknesses, the tints, though at the very commencement of the first order, are not the same within as without the poles of no polarisation. In the position of Pl. XVI. fig. 7, however, and when the tint at O, in the position of Pl. XVI. fig. 8, is only blue of the first order, this irregularity of the tints is almost imperceptible, and the system of rings is distinguished from that produced by crystals with one axis only, by a slight degree of ellipticity. This approximation to the

system shown in Pl. XV. fig. 1, arises from the great attenuation of the crystallized plate, which is no longer capable of developing the tints between the poles of no polarisation; and it is only from the elliptical form of the curves that we can in this state recognise the existence of two axes. Even this ellipticity will disappear by a farther attenuation of the plate; and the crystal deprived as it were of one of its axes, appears to act upon light, exactly like beryl or calcareous spar.

The phenomena which have now been explained, I have found, under various modifications which will afterwards be described, in all the crystals that have more than one axis. The irregularities in the tints are, in every case, developed by increasing the thickness of the plate, and affect the rings which are formed at a considerable distance from the poles of no polarisation. They are analogous to the tints seen along the axis of rock-crystal; and as the secondary forces by which they are produced, conceal the regular action of the principal forces, we must abstract these secondary effects in determining the law according to which the legitimate tints are developed.

2d. On the character, the number, and the position of the axes, by which the tints are produced.

The most important physical circumstance which distinguishes the system of rings formed by different crystals, is the magnitude of the arch PP' , or the inclination of the resultant axes, or diameters of no polarisation; for it is from this angle alone that we can deduce the relative intensity of the real axes, if we suppose them rectangular; or their mutual inclination, if we suppose them equal and inclined.

It would occupy too much room to detail the various practical methods to which I have been obliged to resort in determining the position and inclination of the diameters of no polarisation. In some crystals this process is sufficiently simple, as in topaz, mica, nitre, talc, mother of pearl, prussiate of potash &c. but in other crystals it is extremely difficult, particularly when the angle of inclination is great, and when the specimen is small and its crystalline form indistinct.

The following table contains this angle for 49 crystals. The measures were taken with great care, but some of them are only estimated, and others will admit of correction by the use of better specimens than those I was able to procure.

TABLE of the inclination of the resultant axes of forty nine crystals.

Sulphate of nickel, certain specimens	3 0	25 Sugar	50 0
Carbonate of lead	5 15	Sulphate of strontites	50 2
Nitrate of potash	5 20	Murió-sulph. magn. and iron	51 16
Talc	7 24	Sulph. ammon. and magnes.	51 22
5 Mother of pearl	11 28	Phosphate of soda	55 20
Hydrate of barytes	13 18	30 Sulphate of lime	60 0
Mica, certain specimens, about	14 0	Oxynitrate of silver	62 16
Arragonite	18 18	Feldspar	63 0
Prussiate of potash	19 34	Topaz	65 0
10 Gymophane	27 51	Sulphate of potash	67 0
Borax	28 42	35 Carbonate of soda	70 1
Anhydrite	28 7	Acetate of lead	70 25
Sulphate of magnesia	37 24	Citric acid	70 29
— barytes	37 42	Tartrate of potash	71 20
15 Tincal	38 48	Tartaric acid	79 0
Nitrate of zinc, estimated at about	40 0	40 Tartrate potash and soda	80 0
Stilbite	41 42	Carbonate of potash	80 30
Sulphate of nickel	42 4	Kyanite	81 48
Carbonate of ammonia	43 24	Hyper-oxymur. potash	82 0
20 Mica	45 0	Muriate of copper	84 30
Epidote	45 0	45 Epidote, about	87 0
Benzoate of ammonia	45 8	Peridot	87 56
Sulphate of zinc	44 28	Crystallized Chelt.-salts	88 14
— ammonia	49 42	Succinic acid, estimated at about	90 0
		Sulphate of iron	90 0

In examining the preceding table, we cannot fail to be struck with the uniform distribution of the angles over the quadrant; a circumstance which contributes to render this new property of crystals a most distinct and valuable physical character in mineralogy.

Having thus determined the angles of the resultant axes, we must now proceed to ascertain the position, the number, and the relative intensities of the real axes, by the combination of whose action the various tints are produced; and in doing this we shall take *mica* as an example.

M. BIOT has announced it as a demonstrated physical truth, that mica has two repulsive axes, one in the plane of the laminae, and the other perpendicular to the laminae; and that the polarising force of the first is to that of the second as 100 to 677.* As the experiments from which he deduced this system of forces, consist of observations on the tints, merely in the two rectangular directions, AOB, COD, Pl. XV. fig. 4, we cannot admit them as any thing approaching to evidence for the conclusion which he has drawn; for it is possible that the tints developed in the four quadrants, ACO, ADO, CBO, DBO, may be quite incompatible with such an arrangement. I have observed the tints produced by mica in almost every part of the four quadrants; and it follows from the general law of polarisation which I have discovered, that there is no particular system of forces indicated by the phenomena; but

* "Tous ces résultats étant exactement conformes à ce qu'on observe, il est bien vraisemblable que le système des forces polarisantes du mica est ainsi combiné; mais pour *changer cette présomption en certitude*, il faut exprimer par le calcul les effets d'un pareil système, et voir si la marche des teintes, observées sous chaque incidence, s'y conforme exactement." BIOT *Traité de physique*, tom. iv. p. 557.

that various systems may be proposed which are equally consistent with the observed tints. The number and position of the axes of mica, the rectangularity of the axes, and the nature of the forces which emanate from them, as given by M. BIOT, are therefore entirely hypothetical results.

The phenomena of mica may be explained by the following hypothetical arrangements of the axes.

1. *Rectangular axes.*

1st, By *two unequal negative axes*, Oo, AB, Pl. XV. fig. 4, whose relative intensities are as 100 to 677.

2d, By *a negative axis* Oo, and *a positive axis* CD, whose intensities are as 100 to 173.

3d, By *two unequal positive axes*, CD, AB, whose intensities are as 100 to 117.

2. *Oblique axes.*

4th, By *two equal positive axes*, RS, TV, inclined to each other at an angle of $85^{\circ} 26'$ and

5th, By *two equal negative axes* X, Z, inclined to each other at an angle of $42^{\circ} 2'$.

Hitherto we have supposed that the axes from which the forces emanate, are only two in number; but it will appear from a subsequent part of this paper, that any one axis may be resolved into any even or odd number of axes, by which the phenomena may be explained. All that we know, in short, is that a certain polarising force is exerted at a particular point, but we have no means of ascertaining either the number or the direction of the forces of which it is the resultant. Certain physical circumstances, however, which

will afterwards be noticed, (relative principally to the tints, and the coincidence between some of the hypothetical axes and the principal lines in the crystal,) may lead us to prefer these axes to others; but however plausible the grounds of our preference may be, we must take care not to admit these hypothetical deductions among the number of demonstrated physical truths.

We have already stated that M. BIOT has divided crystals into two classes, attractive and repulsive; and that this division is quite hypothetical, even when applied to crystals with one axis. To the case of crystals with two axes, the classification is wholly inapplicable, for it has been shown, that there are no means of ascertaining either the nature, the number, or the position of the axes. Still, however, the terms *positive* and *negative* may, in most cases, be conveniently employed to mark the principal resulting force which the crystal exhibits. Thus, if we suppose O, Pl. xv. fig. 4, which is the middle point between the two nearest poles of no polarisation to be the position of the principal axis, then mica will be negative, because the tints from O to A, from O to B, from P to C, and from P' to D, have all a negative character; and topaz will be positive, because the same tints have a positive character. In the case of crystals, however, where PP is 90° , even this limited application of the terms negative and positive entirely fails, and these crystals cannot be considered as belonging to one class more than to another.

3d. On the general law of the tints for all crystals with one or more axes.

When a crystal has one axis of double refraction, the tints are disposed on the surface of the sphere, in regular concentric circles around the real or resultant axes of the crystal; and the intensity of the tint at any point of the spherical surface, is equal to $\text{Sin.}^2 \phi$, ϕ being the distance of the point from the pole of the axis, and the maximum tint in the equator of double refraction being considered as unity. This value of the tint was deduced by M. BIOT, from experiments on sulphate of lime, rock crystal, Iceland spar, and some specimens of mica, before he was acquainted with the discovery of the system of concentric rings; but it was obviously not entitled to any confidence as a general principle, not only from its having been deduced from such a small number of crystals, but from its not representing the phenomena in the very crystals from which it was deduced. In sulphate of lime, for example, $\text{Sin.}^2 \phi$ is every where erroneous as the value of the tints. This error indeed is very small in particular azimuths when the distance of the tint from the pole, of what M. BIOT calls the axis, exceeds 30° , but in other azimuths, such as that of 90° , the error is enormous, and in the great circle passing through the plane of the laminæ, the phenomena have no connection whatever, with this law. M. BIOT himself perceived the utter incompetency of the expression $\text{Sin.}^2 \phi$ to represent the phenomena in the vertical azimuths, and has expressed the aberrations which he observed in these directions by complicated empirical formulæ. These formulæ, however, though they represent M. BIOT's,

do not at all represent the actual phenomena, for M. BIOT never made a single observation on the tints in the direction of the laminæ, the only direction in which they could be investigated below 90° , from the supposed pole. Had the experiments been made in this way, he would have found that all the observations, instead of being owing, as he supposes, to secondary forces arising from the inequal superposition of the laminæ, are the legitimate results of two axes of double refraction. The experiments of this philosopher upon sulphate of lime must therefore be set aside as incompetent to determine the accuracy of $\text{Sin.}^2\phi$, as the expression of the tints in crystals with one axis.

As rock crystal possesses secondary forces which interfere with the action of the principal axis, and as there is reason for believing that its apparent axis is only the resultant of two equal negative axes, the value $\text{Sin.}^2\phi$ cannot be deduced from the valuation of the tints which it developes.

The experiments made by M. BIOT upon calcareous spar, previous to his knowledge of the coloured rings, were better fitted to afford an expression of the variation of the tints; but when we consider that his mode of observation was such that, when applied to arragonite, it gave $\text{Sin.}^2\phi$ as the law of the tints, and that this mineral has two axes, and cannot therefore have its tints regulated by such a law, we are forced to conclude, that this mode of observation is insufficient even when applied to calcareous spar.

Some new method, therefore, of studying the phenomena of double refraction and polarisation was wanting, in order to determine with certainty, whether any crystal had one or more axes; and what is the law according to which the tints vary in crystals with one axis. By the old mode of observation,

MALUS concluded that *arragonite* and *sulphate of barytes* were crystals with one axis, whereas they have both two axes, and, by M. BIOT's mode of observation, he concluded that *sulphate of lime*, *arragonite*, *topaz*, *sulphate of barytes*, *sulphate of strontian*, and *feldspar*, had only one axis, whereas they have all two distinct axes of double refraction.

The method of observation which I have always employed, consists in observing the system of coloured rings which are seen along the axes of crystals with one axis, and along the resultant axes of crystals with two axes. These rings I first discovered in mica and topaz, about the end of the year 1812, and early in 1813 I discovered in beryl and several other minerals with one axis, the system of rings peculiar to crystals of that class. Dr. WOLLASTON discovered the same system of rings in calcareous spar in 1814; and long after this discovery was made, M. BIOT examined the phenomenon, and showed by measuring the diameters of the rings that in this crystal the variation of the tints was expressed by $\text{Sin.}^2 \phi$. By similar measurements of the rings in *zircon*, *ice*, *beryl*, *emerald*, *sapphire*, *ruby*, *rubellite*, *tourmaline*, *apatite*, *vesuvian*, *mellite*, *nepheline*, *muriate of lime*, *muriate of strontian*, *arseniate of copper*, I have found that the expression $\text{Sin.}^2 \phi$ applies within the limits of the error of experiments to all these crystals, which, excepting *hydrate of magnesia* and *quartz*, are the only crystals known to have but one axis of double refraction. I therefore consider myself as entitled to set out with this formula, as an expression of the tints for all crystals with one axis, whether their action is of a positive or a negative character.

In proceeding to explain the general law which I have discovered for determining the tints in crystals with any

number of axes, let us suppose that ABC, Pl. xvi. fig. 9, represents the quadrant of a spherical surface, such as we have described in p. 266; and that the position of G, one of the resultant axes, where the tint is nothing, has been carefully determined by experiment, it is required to find the tint at any point E by the action of certain polarising forces which are in *equilibrium* at the point G. If we now suppose,

1st. That the tints are produced by forces emanating from *two negative axes* whose poles are C, A, it is obvious that their relative intensities must be in the ratio of $1 : \frac{1}{\sin.^2 GC}$, GC representing half the inclination of the diameters of no polarisation. For as the tint at G produced by A is equal to the tint produced at the same point by C, and since the tint produced there by the axis A is its maximum tint, AG being 90° , then the maximum tint produced by C will be found by the analogy $\sin.^2 GC : \text{Rad.}^2 = 1 : \frac{1}{\sin.^2 GC}$.

2d. If we suppose that the forces emanate from *two positive axes* A, B, A being greater than B, then the relative intensities must be as $1 : \frac{1}{\cos.^2 GC}$.

3d. If the forces emanate from two axes B, C, one of which is positive and the other negative, the intensity of B must be to that of C as $\sin.^2 GC : \cos.^2 GC$.

Through E draw three great circles AEF, BE and CE, Pl. xvi. fig. 9. and let

T = tint required at the point E.

θ = the arch between the point E and the axis C.

ϕ = the arch between the points E and B.

a = the tint produced separately at E by the greater axis.

b = the tint produced separately at E by the lesser axis.

ψ = the angle of the forces.

π = the angle B,EF.

ω = the angle CEF.

A = the arch FC, or the angle CAF, or the *azimuth* on the great circle BGC passing through the poles of no polarisation.

D = the arch FE, or the declination or distance of the point E from the same great circle.

ζ = half the difference of the angles at the base or at the diagonal of the parallelogram of forces. Then

1. When the two axes are B,C in the plane passing through the diameters of no polarisation, we have

$$\text{Cos. } \theta = \text{Cos. } A \times \text{Cos. } D.$$

$$\text{Cos. } \phi = \text{Sin. } A \times \text{Cos. } D.$$

2. When the two axes are C,A in a plane perpendicular to the plane passing through the diameters of no polarisation,

$$\text{Cos. } \theta = \text{Cos. } A \times \text{Cos. } D.$$

$$\phi = 90^\circ - D.$$

Then we have, in general, whether the axes are A,C or B,C

$$\text{Cos. } \omega = \frac{\text{Tang. } D}{\text{Tang. } \theta}.$$

$$\text{Cos. } \pi = \frac{\text{Tang. } D}{\text{Tang. } \phi}.$$

When B,C are the two axes, either both positive or both negative,

$$\psi = 2 \overline{\pi + \omega}.$$

When A,C are the axes either both positive or both negative,

$$\psi = 2(180^\circ - \omega) = 2\omega.$$

When B,A are the axes either both positive or both negative,

$$\psi = 2(180^\circ - \pi) = 2\pi$$

When B,C are the axes, the one positive and the other negative,

$$\psi = 180^\circ - 2 \overline{\pi + \omega} = 2 \overline{\pi + \omega}$$

When A and C are both positive, or both negative, and $C > A$

$$a = \text{Sin.}^2 \text{CE}, \text{ and } b = \text{Sin.}^2 \text{EA} \times \text{Sin.}^2 \text{GC}$$

When A and B are both positive, or both negative, and $A > B$

$$a = \text{Sin.}^2 \text{AE}, \text{ and } b = \text{Sin.}^2 \text{BE} \times \text{Cos.}^2 \text{GC}$$

When B and C are the one positive, and the other negative,

$$a = \text{Sin.}^2 \text{EC} \times \text{Cos.}^2 \text{GC}, \text{ and}$$

$$b = \text{Sin.}^2 \text{BE} \times \text{Sin.}^2 \text{GC}.$$

The tints produced separately by each axis being thus determined, the tint resulting from their joint action will be found to be the diagonal of a parallelogram, whose sides are a, b and whose angle is ψ . In order to find this diagonal,

$$\text{We have } \text{Tang. } \zeta = \frac{a - b \text{Tang. } \frac{1}{2} \psi}{a + b}, \text{ and}$$

$$\zeta + \frac{1}{2} \psi = \text{Greater angle at the base};$$

$$\text{Hence } T = \frac{a \text{Sin. } \psi}{\text{Sin. } (\zeta + \frac{1}{2} \psi)}.$$

$$\text{When } a = b \text{ then } T = 2a (\text{Cos. } \overline{\pi + \omega})$$

When $a = b$, and the axes equal, then $\pi = \omega$ and

$$T = 2a (\text{Cos. } 2\pi) \text{ or } T = 2a (\text{Cos. } 2\omega) \text{ and since } \phi = \theta$$

$$T = 2 \text{Sin.}^2 \phi (\text{Cos. } 2\pi)$$

$$\text{When } \psi = 90^\circ \text{ then } T = \sqrt{a^2 + b^2}$$

$$\text{When } \psi = 180^\circ \quad T = a - b$$

$$\text{When } \psi = 360^\circ \quad T = a + b$$

This general law of the tints may be expressed in the following manner: *the tint produced at any point of the sphere by the joint action of two axes, is equal to the diagonal of a parallelogram, whose sides represent the tints produced by each*

axis separately, and whose angle is double of the angle formed by the two planes passing through that point of the sphere and the respective axes.

If the crystal has three or more axes, the resulting tint produced from any two of them may, in like manner, be combined with the third, and this resulting tint with the fourth, till the general resultant of all the forces is obtained.

If the number of axes with given intensities exceeds *two*, they may be combined by the methods explained in the next section, till they are reduced to two axes, with new relative intensities; and the resultant of all the axes will be obtained by the calculation of the diagonal of a single parallelogram.

The law which we have now explained is obviously deduced from no empirical data, but is rigorously physical, and is founded upon the same principles which regulate the combination of all other mechanical forces. The accuracy with which it represents the complicated system of tints is very wonderful, and cannot fail to recommend it to the reception of philosophers as a true law of nature. In establishing its conformity with the actual phenomena, I shall not content myself with examining it by means of my own experiments. I shall submit it to the severe ordeal of M. Biot's measurements of the tints of *sulphate of lime*, taken long before the discovery of the law, and which he considered as the result of irregular action depending upon imperfect crystallization. To those who may desire farther evidence, a still more decisive trial may be offered; a trial too, in which the eye itself is capable of recognising the perfect identity between the observed and calculated results. If we compute all the tints,

by means of the law, for any crystal in which the rings round the resultant axes can be seen at one view, and project them upon paper, after they are reduced to different thicknesses, corresponding to the oblique transit of the rays through the parallel plate, we shall have a representation of the rings actually observed, expressing in the most accurate manner all the inversions of the tints, and exhibiting the points of contrary flexure, and the innumerable varieties of form which the curves assume.

In order to determine the tints of sulphate of lime at great obliquities, M. BIOT placed the laminæ in a tube shut up at both ends by plates of glass; and by means of a metallic rod he was able to fix them at any angle with the axis of the tube. He then filled the tube with water, or oil of turpentine, and observed the tints at inclinations as high as seventy-eight degrees, two minutes. In this way he constructed a formula which represented these observations, and by means of this formula he computed the following table, in which I have reduced his numbers, in order to represent the tints at equal thicknesses, by dividing them by the secants of the angles of refraction.

TABLE of the tints of sulphate of lime in different azimuths reduced from M. BIOT's experiments.

Angle of refraction	Angular dist. from the axis.	Azimuth of 0°	Azimuth of 22° 30'	Azimuth of 45°	Azimuth of 67° 30'	Azimuth of 90°
0°	90°	1.0000	1.0000	1.0000	1.0000	1.0000
10	80	0.9768	0.9787	0.9838	0.9905	0.9944
20	70	0.9105	0.9164	0.9352	0.9615	0.9756
30	60	0.8101	0.8213	0.8600	0.9117	0.9434
40	50	0.6885	0.7047	0.7664	0.8523	0.9002
50	40	0.5559	0.5751	0.6607	0.7794	0.8482
60	30	0.4241	0.4409	0.5483	0.6982	0.7938
70	20	0.3027	0.3043	0.4121	0.5949	0.7337
80	10	0.2077	0.1599	0.2501	0.4060	0.6184
90	0					

In determining the tints by new experiments, I adopted two different methods of investigation. By one of these, which was analogous to that of M. Bior, I observed the tints for all angles of refraction up to 60° , and by another mode I observed the remaining tints up to 90° , and I endeavoured to avoid as much as possible the secondary effects produced by obliquity.

The first of these methods is shown in Pl. xvi. fig. 11, where ABCD is a plate of sulphate of lime, whose natural surfaces, AB, CD, are parallel to the plane passing through the resultant axes. Upon these surfaces I cemented two prisms of crown glass M, N, by a thin and equal film of Canada balsam, and having placed them upon the goniometer, I was able to observe the tints with great correctness at angles of refraction considerably beyond those which could be obtained in air. At angles of refraction between 60° and 90° , I resorted to the second method, which consisted in transmitting the polarised light through the parallel faces AD, AC. This method is attended with peculiar difficulties; and much perplexing labour must be submitted to before good plates can be obtained, as the laminæ are constantly separating from each other; and by admitting the materials employed in grinding and smoothing the surfaces, the transparency of the plate is destroyed. I found it necessary, indeed, to bind the laminæ together by wax, sometimes by plates of wood, and at other times by placing them in a small hand vice. By these means my experiments at last succeeded, and all the mysterious actions of sulphate of lime, with the origin and classification of which M. Bior had been so much perplexed, were immediately unravelled. The two resultant axes and their attendant rings were observed at an angular distance of 60° ; the relative intensities of the two axes, from which all the irregularities arose, were thus given,

and the actual progress of the tints became a matter of simple observation. In this way I constructed the following table.

Angle of refraction	Angular dist. from the axis.	Azimuth of 0°	Azimuth of $22^\circ 30'$	Azimuth of the resultant axis 30°	Azimuth of 45°	Azimuth of $67^\circ 30'$	Azimuth of 90°
0°	90°	1.0000	1.0000	1.000	1.0000	1.0000	1.0000
10	80	0.978	0.979	0.980	0.984	0.992	0.995
20	70	0.913	0.919	0.927	0.938	0.962	0.973
30	60	0.811	0.824	0.835	0.861	0.913	0.939
40	50	0.689	0.710	0.729	0.768	0.853	0.898
50	40	0.558	0.576	0.588	0.662	0.782	0.850
60	30	0.435	0.438	0.448	0.538	0.719	0.810
70	20	0.340	0.294	0.304	0.410	0.659	0.775
80	10	0.274	0.125	0.154	0.305	0.614	0.760
90	0	0.250	0.105	0.000	0.246	0.597	0.753

In the preceding table I have not given the value of the tints to more than three decimal places, as even the third decimal place is partly the result of estimation.

To those who may repeat these experiments, it will be necessary to state, that in almost all crystals with two axes, the tints in the neighbourhood of the resultant axes, when the plate has a considerable thickness, lose their resemblance to those of NEWTON's scale, as will be more minutely described in another paper. The rings, however, are perfectly formed; and the numbers in the table are the values of the tints deduced from their position, and not from their actual colour. Thus, in the third ring or order of colours, reckoned from the resultant axes, I call the value of the middle point about 17, although the tint is not a yellowish green, as in NEWTON's scale. This mode of proceeding is strictly correct, for the cause which prevents the tint from being a yellowish green, disappears in general by diminishing the thickness of the plate.

In comparing these observations with the general law, we shall suppose that one of the axes has the same situation in

the laminæ as the line which M. BIOT calls the axis,* when the other axis is perpendicular to the plane of the laminæ. These positions are indicated in Pl. xv. fig. 4, by O and A, and in fig. 9, by C and A. Since GC, therefore, or half the distance of the resultant axis is 30° , we shall have the relative intensity of the axis C, to that of A, as $\frac{1}{\sin.^2 GC}$: or as 4 to 1. With these data the following table has been calculated.

Angle of re- fraction.	Angular distance from the axis.	Azimuth of 0° .	Azimuth of $22^\circ 30'$.	Azimuth of the resultant axis, 30° .	Azimuth of 45° .	Azimuth of $67^\circ 30'$.	Azimuth of 90° .
0°	90°	1,0000	1,0000	1,0000	1,0000	1,0000	1,0000
10	80	0,9775	0,9796	0,9811	0,9848	0,9903	0,9925
20	70	0,9124	0,9202	0,9258	0,9401	0,9614	0,9707
30	60	0,8125	0,8272	0,8385	0,8660	0,9160	0,9375
40	50	0,6901	0,7093	0,7254	0,7729	0,8573	0,8967
50	40	0,5600	0,5755	0,5936	0,6593	0,7901	0,8533
60	30	0,4375	0,4356	0,4507	0,5339	0,7221	0,8125
70	20	0,3378	0,2980	0,3018	0,4071	0,6615	0,7792
80	10	0,2726	0,1238	0,1511	0,2983	0,6190	0,7575
90	0	0,2500	0,1035	0,0000	0,2500	0,6036	0,7500

The agreement between the numbers in this and the preceding table, will convey an idea of the accuracy with which the law accommodates itself to all the capricious changes of the tints. I have compared it also with numerous experiments made on *nitre*, *mica*, *topaz*, *sulphate of iron*, &c. embracing all the varieties in the inclination of the resultant axes, and have found it equally accurate in all these crystals. The projection of the calculated results, indeed, compared with the system of rings where their curvature is the most variable, may be considered as an ocular demonstration of the correctness of the law.

* It is a curious fact, that sulphate of lime is the only crystal, out of a great number, in which the principal axis does not coincide with any of the prominent lines in its primitive form, as ascertained by HALL. Does not this prove incontestably that its primitive form is at present undetermined?

SECT. IV. *On the resolution and combination of polarising forces, and the reduction of all crystals to crystals with two or more axes.*

It has been remarked by M. BIOT in one of his latest memoirs, that it has been established by LAPLACE, that if the Huygenian law is admitted, the extraordinary refraction is *necessarily* produced by a repulsive force; and the same philosopher has also asserted, that from the opposite character of the tints produced by beryl and quartz, there results *necessarily* this alternative: " Either the forces which produce the extraordinary refraction are repulsive in one of the classes and attractive in the other, or they are repulsive in both; but they turn the axes of the luminous molecules in directions inversely rectangular. New experiments have proved to me that the first mode is that which nature realises." *

This view of the subject of double refraction and polarisation has always appeared to me erroneous. I have never been able to perceive that the phenomena of calcareous spar were *necessarily* referable to a repulsive force, or that nature had restricted herself to any of the alternatives which have just been stated. These opinions have acquired new strength as I advanced in the inquiry, and I trust I shall be able to demonstrate, not only that the phenomena of double refraction and polarisation may be explained by forces or combinations of forces different from those which have been given by LAPLACE and BIOT, but that there are certain analogies of nature, and certain physical circumstances in the phenomena, which

* *Mem. de l'Institut. Lu, 2 Jan. 1815.*

may lead us to select one combination of forces in preference to others, as the means which nature has employed in the accomplishment of her purposes.

If we consider a material particle in motion as under the influence of forces, the nature and the source of which are unknown, we may ascribe any change of direction which it experiences, either to a single attractive, or a single repulsive force, emanating from different sources; or we may regard it as the resultant of a variety of forces of the same, or of opposite characters. In the phenomena of the solar system, a repulsive force is necessarily excluded by the simplest considerations. In the reflection of light, the return of the ray cannot, without the most manifest absurdity, be ascribed to an attractive force residing without the reflecting surface; and for the same reason, the refraction of the transmitted light cannot be considered as the effect of a repulsive force existing without the transparent medium. But the case is quite different in the phenomena of double refraction and polarisation. There are here no prominent physical circumstances which can lead us to a general determination of the nature of the forces. The deviation of the extraordinary ray in beryl, may be the result of a repulsive force emanating from the axis of the prism, or of an attractive force emanating from two equal rectangular axes lying in a plane perpendicular to the axis of the prism, or of various other combinations of forces, either of the same or of opposite names.

We shall now proceed to demonstrate the first of these positions, namely, that the action of two equal rectangular axes of a positive character, as calculated by the law of polarisation already explained, is the same as the action of

one negative axis of the same intensity as either of the other two, and placed at right angles to the plane of the positive axes.

Let A,B,C, fig. 9, be the poles of the three axes of which B,C are the positive axis, and A the negative axis.

The tint produced at any point E by the axis A alone, is of the same intensity, and the same character, as the tint that would be produced at the same point by the positive axes B,C acting jointly. Through E draw the great circles AEF, BE, and CE, and call $EF=x$, $BE=\phi$, $EC=\theta$, $BF=m$, $CF=n$, $BEF=\pi$, $CEF=\omega$. Then as the tint produced at E by the axes A alone is $\text{Sin.}^2 AE$ or $\text{Cos.}^2 x$, we must show that the resulting tint produced by the two axes B,C, is also equal to $\text{Cos.}^2 x$. By spherical trigonometry we have

$$\text{Sin.}^2 \phi \times \text{Sin.}^2 \pi = \text{Sin.}^2 m$$

$$\text{Sin.}^2 \theta \times \text{Sin.}^2 \omega = \text{Sin.}^2 n$$

But since $m + n = 90^\circ$ $\text{Sin.}^2 n = \text{Cos.}^2 m$. Hence

$$\text{Sin.}^2 m + \text{Sin.}^2 n = 1$$

and by adding together the two first equations we have

$$\text{Sin.}^2 \phi \times \text{Sin.}^2 \pi + \text{Sin.}^2 \theta \times \text{Sin.}^2 \omega = 1.$$

Again, since $\text{Tang.} \phi = \frac{\text{Sin.} \phi}{\text{Cos.} \phi}$, and $\text{Tang.} x = \text{Tang.} \phi \times \text{Cos.} \pi$

and $\text{Cos.} \phi = \text{Cos.} x \times \text{Cos.} m$, we obtain by substitution

$$\text{Tang.} x = \frac{\text{Sin.} \phi \times \text{Cos.} \pi}{\text{Cos.} x \times \text{Cos.} m} \text{ and}$$

$\text{Tang.} x \times \text{Cos.} x \times \text{Cos.} m = \text{Sin.} \phi \times \text{Cos.} \pi$; but since

$\text{Tang.} x \times \text{Cos.} x = \text{Sin.} x$ we have

$\text{Sin.} x \times \text{Cos.} m = \text{Sin.} \phi \times \text{Cos.} \pi$, and by the same reasoning

$$\text{Sin.} x \times \text{Cos.} n = \text{Sin.} \theta \times \text{Cos.} \omega.$$

Hence, after squaring both equations and adding them together, we have

$$\text{Sin.}^2 x \times \text{Cos.}^2 m + \text{Sin.}^2 \pi \times \text{Cos.}^2 n = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega$$

$$\text{and Sin.}^2 x = \frac{\text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega}{\text{Cos.}^2 m \times \text{Cos.}^2 n}$$

$$\text{But Cos.}^2 m \times \text{Cos.}^2 n = 1. \text{ Hence}$$

$$\text{Sin.}^2 x = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega.$$

$$\text{But since Cos.}^2 \pi = \text{Cos.}^2 \pi + \text{Sin.}^2 \pi \text{ and}$$

$$\text{Cos.}^2 \omega = \text{Cos.}^2 \omega + \text{Sin.}^2 \omega \text{ we have}$$

$$\text{Sin.}^2 x = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \phi \times \text{Sin.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega + \text{Sin.}^2 \theta \times \text{Sin.}^2 \omega$$

$$\text{But Sin.}^2 \phi \times \text{Sin.}^2 \pi + \text{Sin.}^2 \theta \times \text{Sin.}^2 \omega = 1; \text{ hence}$$

$$\text{Sin.}^2 x = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega + 1, \text{ and}$$

$$\text{Since Cos.}^2 x = 1 - \text{Sin.}^2 x \text{ we have}$$

$$- \text{Cos.}^2 x = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega$$

Now in the parallelogram of forces, the two forces AC, AD (Pl. xvi. fig. 12) are $\text{Sin.}^2 \phi$, $\text{Sin.}^2 \theta$ when the axes are of equal intensity, that is $AC = \text{Sin.}^2 \phi$, and $AD = \text{Sin.}^2 \theta$. But

$$\text{Sin.}^2 \phi : \text{Sin.}^2 \theta = \text{Sin.}^2 \pi : \text{Sin.}^2 \omega, \text{ and the}$$

$$\text{Angle CAD} = 2\pi + 2\omega. \text{ Hence}$$

$$\text{CAB} = 2\pi \text{ and}$$

$$\text{BAD} = 2\omega$$

$$\text{Consequently AF} = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi \text{ and}$$

$$\text{BF} = \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega, \text{ and therefore}$$

$$\text{AF} + \text{BF} = \text{AB} = \text{Sin.}^2 \phi \times \text{Cos.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.}^2 \omega$$

which is the very same value which we have found above for $\text{Cos.}^2 x$, the measure of the tint produced by one axis.

Hence it follows that the intensity of the tints, and consequently the form of the curves of equal tint, are the same, whether they are produced by one negative, or by two equal and rectangular positive axes.

We may therefore conclude,

1st. That a single negative axis may be resolved into two

equal rectangular positive axes of the same intensity as the negative axis, and lying in a plane perpendicular to that axis.

This fundamental principle being established, we may proceed still farther in the resolution of the axes into axes of equal intensity, but differing in their character and position. Thus in Pl. xvi. fig. 9, the negative axis A being equivalent to the positive axes B, C, we may again resolve B into a negative axis at C and another at A. Hence the original axis at A is equivalent to a negative axis at A, a positive axis at C, and a negative axis at C. But these two last axes destroy each other; and therefore the negative axis A is the only one that is left. If we also resolve the other positive axes C into two negative axes at A and B, we have the original negative axis A, equivalent to four negative axes, two at A, one at C, and one at B. We therefore conclude,

2d. That the effect of a single negative axis may be represented by three rectangular negative axes, provided two of them are equal, and the third has a greater intensity than the other two; and *vice versa* a single positive axis may be represented by three rectangular positive axes, provided two of them are equal, and the third has a less intensity than the other two.

Hence it follows that by leaving one axis at A, the effect of the other three negative axes must be to destroy each other, a result which will afterwards be established in a different way, and will lead us to some important consequences. The same is true of positive axes, *mutatis mutandis*.

Hitherto we have resolved the axes of crystals into other axes differing in position and character, but having the same intensity. If we consider, however, that any one axis at A

is equal to any number of equal or inequal axes of the same name placed at A, the sum of whose intensities is equal to the intensity of the axis at A, it will follow—

3d. That the effect of a single axis may be represented by any even number of equal axes of an opposite name, provided in every pair of axes one of them is at right angles to the other, and all of them lie in the same plane perpendicular to the original axis. The intensity of each of the new axes must be to that of the single axis as $\frac{n}{2}$ to 1, n being the number of axes.

4th. That the effect of a single axis may be represented by any number of equal pairs of axes of an opposite nature, provided the axes which compose each pair are equal and rectangular, and all of them lie in the same plane perpendicular to the original axis. The intensity of the axes which compose each pair may differ in any way, but $\frac{1}{2}$ or half the sum s of the intensities of all the axes must be equal to the intensity of the single axis.

In the preceding observations we have supposed the axes of the crystals to be rectangular, a supposition which is by no means rendered necessary by the phenomena. It is obvious indeed that the effect of a single axis cannot be represented by two axes that have any other inclination but that of 0° or 90° ; but we shall presently see that the effect of two rectangular axes may be represented by two equal and inclined axes. Let ABC, Pl. xvi. fig. 10, be a spherical triangle representing the eighth part of the sphere, and having all its angles right angles. Then if G is the pole of one of the resultant axes or diameters of no polarisation, the angular distance $\alpha\beta$ of the two inclined axes α, β must be such, that the angle $\alpha G\beta$ is a

right angle, for it is only at this angle that the two axes can destroy each others' actions, and produce a resultant axis at G. Consequently, since αGF is 45° , we have $\text{Tang. } \frac{1}{2} \alpha \beta = \text{Sin. GF.} = \text{Cos. AG.}$ The same reasoning is applicable if the axes are situated at a, b , $a G b$ being a right angle; but in this case $\text{Tang. } \frac{1}{2} a b = \text{Sin. AG.}$ Hence,

5th. The action of two unequal rectangular axes is equal to the action of two equal axes, the tangent of the half of whose inclination is equal to the cosine of half the angle of the resultant axes, if the two equal axes are situated in the plane BC, or to the sine of half that angle if they are situated in ab .

In order to ascertain the intensity of the axes α, β , or a, b , we must assume a position and a character for the rectangular axes which they represent. Let the two rectangular axes, therefore, be a negative axis at A, and a positive axis at F, whose relative intensities will be $\text{Cos.}^2 GA : \text{Sin.}^2 GA$, and let A be the most powerful axis. Then the intensity of either of the axes α, β will be to that of A as $\frac{1}{2 \text{Sin.}^2 \frac{1}{2} \alpha \beta} : 1$; and the intensity of either of the axes a, b will be to that of A as $\frac{1}{2 \text{Cos.}^2 \frac{1}{2} a b} : 1$. Hence when $\alpha \beta = 90^\circ$, we have $\frac{1}{2 \text{Sin.}^2 \frac{1}{2} \alpha \beta} = 1$, which brings us back to Case 1st. When $a b = 0^\circ$, we have $\frac{1}{2 \text{Cos.}^2 \frac{1}{2} a b} = \frac{1}{2}$, for as the two axes a, b coincide with A, their sum must be equal to A, or $a + b = 1$, and since $a = b$, we have $a = \frac{1}{2}$. When B is the most powerful axis, the preceding ratios of the intensities must be interchanged.

If the two inclined axes α, β are supposed to be unequal, they may have an infinite number of positions in the great circle passing through α, β ; but their relative position must be such, that the great circles passing through each of them,

and the resultant axis must intersect each other at an angle of 90° , and their intensities will be as the squares of the sines of their respective distances from the resultant axis. The ratio of the intensities, however, can never exceed that of 1 to $\text{Sin.}^2 \phi$, ϕ being the distance of the resultant axis from the plane passing through α, β .

It would be needless to pursue this subject any farther. I have briefly illustrated the general principles of the resolution of the axes of crystals, and the reader will have no difficulty in deducing other combinations by which the phenomena may be represented. A very important question is naturally suggested by the results to which we have arrived. Are there any physical circumstances either of a general or a particular nature which may lead us to ascertain the real position of the axes of crystals, and to determine the character of the forces by which the phenomena of polarisation are produced? When we consider the case of *Iceland spar*, we perceive no peculiarities which can induce us to refer its polarising force to two or more positive axes in preference to a negative axis; but in *rock crystal*, the secondary tints discovered by M. BIOT along the axes of the prism, seem to indicate that its apparent positive axis is merely the resultant of two or more equal and rectangular negative axes. M. BIOT indeed ascribes these secondary effects to new forces independent of the principal polarising force; but I have discovered them also in crystals with two axes, and have observed some phenomena which seem to prove that they have their origin in the unbalanced action of the two principal axes.

With regard to the nature of the forces we are not left

entirely without some general indications. In magnetism and electricity, the various phenomena are produced by two opposite and co-existent forces which modify each other's action; and since opposite forces are obviously indicated by the phenomena of polarisation, we have the strongest reasons, from analogy, to believe that they are also co-existent in crystals. The remarkable phenomena exhibited during the transmission of heat along plates of glass, give additional weight to the deductions of analogy. Here the negative structure invariably accompanies the positive structure, and the plates which are under the influence of these forces, exhibit phenomena precisely the same as artificial magnets.* It will, however, be naturally asked, what has become of the negative force in calcareous spar, if it is supposed to have two equal and rectangular positive axes? To this we reply, that the negative force, originally less than the positive forces, has been balanced by the opposite actions of the two positive axes; and that the resulting force, which has all the characters of a negative force, is merely the difference between the original negative force and the negative force which is equivalent to the effect of the positive axes. Nor is this mere speculation, for while it is an arrangement of the polarising forces which is as likely as any other to be the one actually followed by nature, it is almost directly supported by a series of experiments, which I have made on plates and cylinders of glass.†

* *Phil. Trans.* 1816, p. 64, 83, 84, &c.

† See the *Edinburgh Transactions*, vol. VIII. p. 353.

SECT. V. *On the polarising structure of crystals that have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive form.*

In examining the structure of doubly refracting crystals, it will be found that there are *thirteen* bodies which seem to be entirely destitute of the polarising structure, and other *nine* which sometimes exhibit distinct traces of two opposite structures.* And it is a very curious circumstance, that the crystals which have this character are those which crystallize in the form of a cube, a regular octohedron, and a rhomboidal dodecahedron, as appears from the following table.

<i>Names of crystals.</i>	<i>Primitive form.</i>	<i>Names of crystals.</i>	<i>Primitive form.</i>
Garnet	Rhomboidal dodecahedron	Boracite†	Cube
Blende	Rhomboidal dodecahedron	Muriate of soda	Cube
Diamond	Regular octohedron	Muriate of potash	Cube
Spinnelle ruby	Ditto	Leucite	Cube
Ceylanite	Ditto	Analcime	Cube
Alum	Ditto	Nitrate of barytes	
Muriate of ammonia	Ditto	Sulphate of alumine and ammonia	
Fluor spar	Ditto	Nitrite of lead	
Ruby copper	Ditto	Cinnamon stone	
Nitrate of lead	Ditto	Essonite	
Nitrate of strontian	Octohedral crystals	Uranite	

These crystals, as will be seen by a comparison with HAÛY's table of primitive forms,‡ include all the transparent and translucent crystals which he has arranged under the preceding forms, excepting *tungstate of lime*; but this ex-

* See the *Edinburgh Transactions*, vol. VIII. p. 157, where I have given an account of the discovery of this property.

† Boracite has an axis in every direction, like the waxen partitions of the honey-comb.

‡ *Traité de Minéralogie*, tom. i. p. 273.

ception is removed, as he has since found that this mineral has for its primitive form an irregular octohedron.

In the paper already quoted, I have described the leading optical properties of this class of crystals; but I have since observed several new phenomena which throw additional light on the subject. In the examination of thirty very fine cut diamonds of great size and value, I found several which had no action whatever upon light. Many of them polarised a fine sky blue tint of the first order, with a complementary straw yellow, in whatever position they were held; and one exhibited a succession of five or six tints, which were not the exact tints of NEWTON's scale, but similar to those seen near the resultant axes of crystals. In a diamond with natural faces, which has the form of a fine regular octohedron, there were no indications whatever of the polarising structure. In two specimens of *leucite*, broad segments of the coloured rings were developed when the light was transmitted in different directions; and *blende* and *analcime*, even at thicknesses less than $\frac{1}{25}$ of an inch, displayed a considerable action upon polarised light.

M. BIOT has endeavoured to account for the absence of polarisation and double refraction in this class of crystals by remarking—"that the phenomena of the attractive class are explicable by a prolate ellipsoid and those of the repulsive class, by an oblate ellipsoid; and that the sphere, forming a passage from one of these limits to the other constitutes a sort of neutral state, and corresponds to those crystals which, crystallizing in the regular octohedron or the cube, are destitute of the property of double refraction."*

* *Traité de Physique*, tom. iv. p. 349, Paris, 1816.

This remark, however, is by no means an explanation of the fact. It is merely a different mode of expressing the fact, that the cubical and octohedral crystals have no double refraction; for the sphere is applicable to the optical action of air and all other fluids, whether aeriform or liquid, which have not the property of double refraction.

The discovery of the remains of polarising axes in a great number of crystals of this class, completely proves that, like all other crystallized bodies, they actually possess the doubly refracting and polarising structure, but that this structure entirely vanishes in some specimens, by the equilibrium of the forces in every point of the crystal, and reappears in some specimens when that equilibrium is not complete.

The principles of the resolution of polarising forces which we have already explained, indicate the manner in which this equilibrium is effected. Three equal and rectangular axes, either all of the negative, or all of the positive character, will produce a perfect equilibrium, or mutually destroy each other at every point of the crystal; for since two equal positive axes are equivalent to one negative axis, of the same intensity as either of the two; and since this negative axis will be balanced by an equal positive axis coincident with it, it follows, that the three rectangular positive axes will be in equilibrio. In perfect crystals of this class where all the axes are equal, and their position exactly rectangular, the equilibrium of the forces is complete; but if the axes are not equal, or their position not accurately rectangular, the phenomena of one or more axes will be developed. If one axis, for example, is weaker than the other, then the result will be the appearance

of an axis of an opposite name to that of the three axes and coinciding with the resultant of the other two; but if one of the axes is stronger than the other two, the result will be an axis of the same name with the three axes, and coinciding with the strongest axis.

Let us now consider what will be the effect if the three negative axes C, A, O, Pl. xv. fig. 4, are all different; C being the strongest and O the weakest. The two axes C and A will, by their joint action, produce resultant axes P, P' lying in the plane COD. Let the third axis O be resolved into two positive axes at C and A, then we have a negative axis A, and a positive axis equal to O acting at A, and the negative axis C, and a positive axis equal to O acting at C; but O is less than A or C, therefore we have a negative axis equal to $A - O$ acting at A, and a negative axis $C - O$ acting at C; and as the last of these is the strongest, the general effect will be the production of two resultant axes in the plane COD. But as $C - O$ has to $A - O$ a greater ratio than C has to A, the poles of no polarisation P, P' will be removed farther from O than they were by the action of C and A alone.

The equilibrium of the three axes may also be disturbed by another cause, namely, by a deviation from perfect rectangularity. In order to understand the effect of this irregularity, let ABC, Pl. xvi. fig. 10, be the three rectangular axes, and $B\alpha$, $C\beta$ the deviation of the axes α and β . Then the effect of the equal and inclined axes α , β is to produce poles of no polarisation, one of which is seen at G in the plane FGA, bisecting the angle formed by the planes passing

through the axes. If the third axis is rectangular to α and β , or placed at A, the effect of it would be to throw the pole G farther from A towards F; but as it is not probable that the deviation of α and β would be in a plane exactly at right angles to A, while A kept its situation; then if A deviates from A, or if while A retains its position, the deviations of α and β are not in the plane BC, an irregular system of curves will be produced, differing essentially from those which are formed by two axes.

Here then we have three leading causes of the destruction of the equilibrium of the three axes. In consequence of the first, tints of a single structure will be developed, as in some specimens of diamond and aluni. In consequence of the second, the opposite tints of a double structure will be developed, as in several specimens of diamond, fluor spar, muriate of soda, semiopal, and analcime; and in consequence of the third, a confused system of rings will be exhibited, such as I have discovered in several specimens of leucite.

The coincidence of the preceding deductions, with the experimental results, affords a strong presumption, that we have been tracing the actual operations of nature. In the phenomena of crystals both with one and with two axes, we have seen that these axes are coincident with some permanent line in the primitive or secondary form of the crystal. This is the case in every crystal with one axis that I have examined; but it is not easy to establish this coincidence in crystals with two axes, for it is impossible to fix the situation of the axes by which the phenomena are produced. In almost every case, however, the most probable position of the axes is

coincident with some prominent line, either in the primitive or secondary forms.*

The same coincidence ought therefore to be expected in the case of the cubical, octohedral, and rhomboido-dodecahedral crystals; and it is a singular fact, that the *cube*, the *octohedron*, and the *rhomboidal dodecahedron*, are the only regular geometrical solids in crystallography, in which neither more nor less than three equal rectangular axes can be placed symmetrically. In the cube, for example, each of three axes is perpendicular to the three pair of square surfaces by which the solid is contained. In the *regular octohedron* each of them coincides with the line which joins the six solid angles of the figure; and in the *rhomboidal dodecahedron*, each of the rectangular axes passes through the six solid angles, each of which is contained by four acute angles of the rhomboidal planes.

SECT. VI. *On the artificial imitation of all the classes of doubly refracting crystals by means of plates of glass.*

In the Philosophical Transactions for 1816, I have given a full account of the very remarkable phenomena which are

* Malus seems to have believed that the axis of extraordinary refraction was necessarily coincident with some prominent line in the primitive form. We quote the following passage in confirmation of our general views, though there can be no doubt that the generalisation which it implies is premature. “ Dans le rhomboïde, l’axe de réfraction se confond avec l’axe du crystal; mais dans les autres formes on n’a pas de données suffisantes pour le déterminer à priori. Cependant le nombre des directions entre lesquelles on peut balancer est toujours très-borné. Dans l’octaèdre à triangle scalène, par exemple, on est assuré d’avance que l’axe de réfraction est un des trois axes rectangulaires de la forme primitive.” *Théorie de la Double Réfraction*, p. 177.

exhibited during the propagation of heat along plates of glass and other uncrystallized bodies; and of the method of communicating permanently to these substances, all the properties of regular crystals. Since that paper was written, I have completed a series of new experiments on the distribution of the polarising force in prepared glass, which led me to the determination of the laws by which all the phenomena can be calculated. These laws are precisely the same as those which we have already deduced in the preceding sections; and there is not one phenomenon belonging to regular crystals which I have not been able to imitate in glass.

In these artificial crystals, however, the centre of the sphere is infinitely distant, and therefore the sine of the angle which the refracted ray makes with the axis in the regular crystals, is equivalent in the artificial ones to the distance of the ray from the axis, and is replaced by this quantity in the new formulæ. Thus if T is the tint corresponding to any given distance D from the axis, then the tint t at any other distance, d is $t = \frac{Td^2}{D^2}$ a formula which represents accurately the progress of the tints whether the axis is negative or positive.

If in rectangular plates of glass D represents the distance of any of the black fringes, or the lines of no polarisation MN, OP , Pl. xvi. fig. 13, from the axis ab of the plate, and T the maximum central tint; then if we conceive an axis situated perpendicular to the plate in every point of the line ab , and an axis in the plane of the plate perpendicular to the former axis, T will be the tint produced in every part of the plate by the axis in its plane when polarised light is transmitted through it perpendicularly. But as this tint, whose value is

T, is counteracted at m in every part of the line MN, by the action of the axes at A, &c. perpendicular to the plate, the intensity of the perpendicular axis at the distance $A m = D$, will also be equal to T. Hence, in order to find the tint t produced by this perpendicular axis at any other distance $A c = d$, we have $t = \frac{T d^2}{D^2}$ by the law for one axis; but as this tint is counteracted by the tint T produced at c by the axis in the plane of the laminæ, we have the resulting tint or $t = T - \frac{T d^2}{D^2}$.* Here the law is very simple, and the tints are all disposed in straight lines, in consequence of the angle of the forces being 180° , and the number of the axes infinite. This case is precisely the same as that of regular crystals when the tints are calculated in the plane COD, Pl. xv. fig. 4, by means of two axes at O and A, the effect of the axis A is the same at every point in the line COD, and therefore the resulting tint, at any point, is the uniform tint produced by A (which is the same as the tint at O) minus the tint produced by the axis O alone. The law of the progression of the tints therefore in rectangular plates of glass, is exactly the same as in crystals with two axes; and we have the same difficulty in determining whether the axes are of the same or of opposite names.

The similarity between the various phenomena of the real

* This formula is the same as that which I had deduced long ago from experiment. M. BIOT had also obtained from observation the very same expression of the tints, and about the same time. The formula which I have employed was more general than that of M. BIOT, as I had found the term D to be a function of the breadth of the plate. My formula was therefore $t = T - \frac{T d^2}{.312 B^2}$, B being the breadth of the plate; or if T is the maximum tint given by the plate, we have $t = T - \frac{T d^2}{.188 B^2}$. See *Edinburgh Transactions*; Vol. VIII. Art. XVIII.

and the artificial crystals is still more striking than the agreement of the laws of the tints, as all the different classes of crystals can be imitated in glass. This similarity will appear from the following comparison :

*Artificial crystals.**Real crystals.*

- | | |
|---|---|
| 1 A circular plate of glass crystallized by the transmission of heat. | 1 Crystals of zircon, quartz, ice, &c. |
| 2 A circular plate of glass heated, and in the act of cooling. | 2 Crystals of calcareous spar, beryl, ruby, &c. |
| 3 A rectangular plate of glass when heat is in the act of being transmitted through the plate. | 3 Crystals of topaz, sulphate of lime, &c. in which the principal force is attractive. |
| 4 A rectangular plate of glass heated, uniformly, and in the act of cooling. | 4 Crystals of mica, nitre, &c. in which the principal force is repulsive. |
| 5 A circular plate of glass, permanently crystallized to a slight degree, and then heated, so as to produce, in the act of cooling, a tint equal to its permanent tint. | 5 Cubical, octohedral, and rhomboido-dodecahedral crystals, in which the polarising force is destroyed by opposite actions. |

Although these artificial crystals exhibit in the most perfect manner all the phenomena of fixed and moveable polarisation, and develop the tints of NEWTON'S scale with much more regularity than even sulphate of lime, yet M. BIOT has described the phenomena which they exhibit under the head of *imperfect crystallization*. If such a distinction were neces-

sary for the proper classification of the phenomena, I would have no hesitation in reversing the name, and giving the appellation of imperfect crystallization to the phenomena produced by *sulphate of lime*, and the greater number of crystals of the mineral kingdom. If we employ pieces of pure glass of regular forms, and with straight and smooth edges, and if we transmit through them the heat in a uniform and careful manner, we shall obtain a structure which will develop all the phenomena of polarisation with the utmost beauty and perfection.

There is one point, however, and a very instructive one, in which the artificial differ from the real crystals. If we cut a plate of calcareous spar into any number of pieces, each piece, however minute, will produce the system of coloured rings in as great perfection as the whole plate of which it formed a part; and its polarising force will suffer no diminution by the reduction of its size, provided its thickness remain the same. If we subdivide, however, a circular plate of glass, the polarising force of each portion is not only much diminished, and sometimes even destroyed, if the portion is very small, but the polarising influence is distributed in a new manner, according to the outward shape of the fragment.* Hence it follows, that in glass the polarising structure *depends entirely on the external form of the plate*, and on the mode of aggregation of its particles. When its form is circular, it has only one axis of polarisation; and this axis is positive, if the density diminishes towards the centre, and negative, if it increases towards the centre; but when

* See *Phil. Trans.* 1816, pp. 71, 82.

its form is rectangular, or elliptical, it has two axes of polarisation, the strongest of which appears to be positive, and the weakest negative.

In crystals, on the contrary, the polarising structure is wholly independent of their outward form, and of any variation of density in the aggregation of their particles, and must therefore depend on the form of their integrant molecules, and the variation in their density. Is it not more than probable, therefore, that we have in our artificial crystals, a representation of the ultimate particles of crystallized bodies? When these crystals have a spherical form, diminishing in density towards an axis, and have these axes arranged by the laws of crystallization, they will constitute a crystal of the positive class, like *ice*, *zircon*, *quartz*, &c.; and the only difference between ice and water will be, that in the ice the particles have their axes regularly disposed; while in the water, the axes have every possible direction, so as to create a general equilibrium of the polarising forces. When the density of the spheres increases towards their axes, their symmetrical combination will constitute a crystal of the negative class, such as *beryl*, *calcareous spar*, &c.

But if the particles, instead of being spheres, are either prolate or oblate spheroids, having their polarising axis at right angles to the axis of revolution; and if their density varies, as in elliptical plates of glass, along which heat is in the act of being transmitted, they will constitute by their regular combination, the class of crystals with two axes, such as *topaz*, *sulphate of lime*, &c. in which the positive force predominates. If the density, on the other hand, varies as in

elliptical plates heated in oil, and in the act of sudden cooling, the combined spheroids will constitute the class of crystals like *mica*, *nitre*, &c. in which the negative force predominates.

The variation of density which these solids necessarily require, in order to develop a force varying as the fourth power, or an influence upon the tints varying as the square of the sine of the angle which the ray forms with the axes of the crystals, will be understood from Pl. xvi. fig. 14. Let $MaNb$ represent the section of the sphere through one of its great circles, then if we suppose that the density is a maximum in every point of the line MN , and that it diminishes from M to ab , and from L to dc , at first very slowly, and then very rapidly, like the cosines of the angles reckoned from MP , so that the lines of equal density in every section of the sphere passing through MN are diameters, the sphere will represent one of the integrant particles of the positive crystals, like *ice*, *zircon*, *quartz*, &c. having MN for its axis of double refraction and polarisation. In like manner, if the density is a minimum in the line MN , and increases in a similar manner, towards ab and cd , the sphere will represent an integrant particle of the negative crystals, such as *beryl*, *calcareous spar*, &c. When the polarised ray is transmitted along the axis MN , the polarising force will be nothing; when it passes along RS , the variation of density is greater, and consequently the polarising force will be increased; and when it is transmitted along ab , the polarising force will be a maximum, as the variation of the density is a maximum in that line. As the same is true of every other section of the sphere passing through MN , it follows that there will be a

certain rate of variation in the density at which the polarising force will be proportional to the fourth power, or the variable influence upon the tints to the square of the sine of the angle which the polarised ray forms with the axis.

The preceding reasoning must not be considered as mere speculation, for such a crystal as that represented in fig. 14, may be actually constructed with plates of glass. Let ABCD, for example, represent in section, a circular plate of glass whose thickness is ML, and along whose axis MN, is seen the system of coloured rings shown in Pl. xv. fig. 1. Then, if we conceive the plate bent back into the position *a M bc L d*, we have the external spherical stratum of our elementary sphere; and in like manner we may conceive the interior strata to be formed by a succession of circular plates DEFC, &c. bent into the spherical form *d L c f K e*. Now, since the tints of the circular plate ABCD vary as the squares of the distances from its axis ML, we may suppose that the same law still exists, after it is bent into *a M bc L d*. But the distances from the axis MN are now the sines of the angular distances from M; and therefore, since the same is true of all the other spherical strata, of which the elementary sphere is composed, it follows that the tints produced by the transmission of polarised light, along any diameter of the sphere, are proportional to the square of the sine of the angle, which the ray forms with the axis of the crystal. In order to construct this sphere artificially, we have only to crystallize a series of hemispherical strata, and join them together at the line *ab* in the manner represented in the figure. This sphere, it will be readily seen, is totally different from a solid sphere of crystallized glass, which has no particular axis, but which gives

the same phenomena by transmitting polarised light through any of its diameters.*

In the very same manner we may suppose the elementary spheroid of crystals with two axes to be formed by elliptical plates, bent into spheroidal strata, and the spheroid itself may be actually constructed by means of spheroidal strata of glass. When this is done, it will exhibit all the complicated phenomena produced by the simultaneous actions of two unequal axes.

SECTION VII. *On the laws of double refraction in crystals with any number of axes.*

From the coincidence which I have invariably observed between the axes of polarisation and extraordinary refraction, in all crystals that have only one apparent axis, we may consider it as an established fact, that the HUYGENIAN law represents, within the limits of experimental errors, not only the phenomena of calcareous spar, but those of all other crystals with a single axis of double refraction.

But since it is the general character of crystals to have more than one axis of polarisation, it becomes interesting to ascertain, if they also have two axes of extraordinary refraction; and if they have, to investigate, by direct experiment, the general laws of double refraction, for crystals with any number of axes. Such an investigation presents difficulties of no ordinary kind. The want of a transparent mineral with two powerful axes, which, like calcareous spar, could be obtained in large pieces, and cut with facility in every direc-

* See *Phil. Trans.* 1816, pp. 314, 315.

tion ; and the necessity that it should have its resultant axes considerably inclined to each other, in order to obtain a measurable separation of the images at several points, between these resultant axes, rendered all my experiments for a long time completely unsuccessful. The discovery, however, of crystals which possessed, in some degree, the most important of these requisites, has enabled me to resume and to complete the investigation.

After ascertaining the position of the diameters of no polarisation in a crystal with two axes, I formed a prism with a great refracting angle, so as to have a flat surface as perpendicular as possible to one of these diameters. I then placed this prism upon the goniometer, and having marked the position when the point of no polarisation, or the centre of the rings (namely P, Plate xv. fig. 4.) was coincident with the polarised image of a candle, I substituted the direct image of a candle, and observed that this image was single, and therefore that the force of double refraction, as well as the polarising force, had completely vanished. Upon turning the goniometer to both sides of these positions, the deviation of the extraordinary ray became perceptible and gradually increased ; and in continuing to observe this deviation in the plane COD passing through the resultant axes, I found that it increased to the middle point O between these axes, where it became stationary ; and that it again gradually diminished towards the other resultant axis P' where the image again became single. I now measured the deviation of the extraordinary ray at the points O, A, C, and at various other points, both in and out of these rectangular directions, and I found that the force of double

refraction, varied in the same ratio as the polarising force, and that all the phenomena, whatever be the number of axes by which they are produced, may be calculated by the same general law which we have already established for the phenomena of polarisation.

Let it be required, for example, to determine the velocity of the extraordinary ray in a crystal with any number of positive and negative axes. By the principles explained in Sect. IV. these axes may be reduced to two equivalent rectangular axes, which may be either of the same, or of opposite names. Let us then take $b =$ the axis of revolution of the two spheroids, a, a' the other axis, β, β' the inclination of the incident ray to the axes of the crystal, ψ the angle of the forces as found in Sect. III. and ζ half the difference of the angles at the base of the parallelogram of forces. Then since the velocity of the light is inversely as the variable radius of the spheroid $\frac{1}{b^2}$ will be the square of the velocity of the ordinary ray, and $\frac{1}{a^2} \frac{1}{a'^2}$ the square of the minimum velocity of the extraordinary ray in virtue of the separate action of each axis. The difference between the squares of the velocities of the ordinary and extraordinary rays will be

$$\left(\frac{a^2 \pm b^2}{a^2 b^2} \right) \text{Sin.}^2 \beta$$

$$\left(\frac{a'^2 \pm b^2}{a'^2 b^2} \right) \text{Sin.}^2 \beta'$$

the sign being positive, when the axis is positive, and vice versa. But as these expressions represent the sides of the parallelogram of forces, we have

$$\text{Tang. } \zeta = \frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta - \left(\frac{a'^2 \pm b^2}{a'^2 b^2} \text{Sin.}^2 \beta' \right) \text{Tang. } \frac{1}{2} \psi}{\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta + \frac{a'^2 \pm b^2}{a'^2 b^2} \text{Sin.}^2 \beta'}$$

Consequently the difference between the squares of the velocities of the ordinary and extraordinary ray produced by the combined action of the two axes will be

$$\frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta\right) (\text{Sin. } \psi)}{\text{Sin. } \left(\zeta + \frac{1}{2} \psi\right)}$$

Hence, calling V the velocity required, we have

$$V^2 = \frac{1}{b^2} \pm \frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta\right) (\text{Sin. } \psi)}{\text{Sin. } \left(\zeta + \frac{1}{2} \psi\right)} \quad \text{and}$$

$$V = \left(\frac{1}{b^2} \pm \frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta\right) (\text{Sin. } \psi)}{\text{Sin. } \left(\zeta + \frac{1}{2} \psi\right)} \right)^{\frac{1}{2}}$$

The form of the compound, or irregular spheroid, may therefore be computed for all doubly refracting crystals.

The general law of double refraction which has now been explained, may be thus expressed.

The increment of the square of the velocity of the extraordinary ray produced by the action of two axes of double refraction, is equal to the diagonal of a parallelogram whose sides are the increments of the square of the velocity produced by each axis separately, and calculated by the law of HUYGENS, and whose angle is double of the angle formed by the two planes passing through the ray and the respective axes.

When the two axes are of equal intensity and of the same character, the preceding law gives the very same results as the law of HUYGENS does for one axis placed at right angles to the other two.

It is scarcely necessary to observe, to those who have studied the preceding sections, that the phenomena of double refraction cannot be referred to the ordinary action of at-

tractive and repulsive forces. If a ray of light is exposed to the ordinary action of a repulsive or an attractive force emanating from two equal and rectangular axes, there is no point of incidence at which the resultant of these forces is nothing, or there is no resultant axis along which the ray ceases to be divided. In like manner, if the ray is exposed to the action of two equal and rectangular axes, one of which is repulsive and the other attractive, there are innumerable points of incidence in which the resultant of the forces is nothing, and these points are situated in the circumference of two great circles perpendicular to the plane passing through the axes, and bisecting the right angle which they form with each other. These, however, are results entirely incompatible with the actual phenomena of double refraction.

In the preceding sections of this letter, I have included only the more general results of my researches, and have reserved, for another paper, an account of my experiments on the absolute polarising forces of crystals; on the form of the ellipsoids by which their double refraction is regulated, and on the position of their axes with respect to their natural faces, or with respect to certain fixed lines in their primitive forms.

In treating of the coloured rings produced by polarised light, I have likewise omitted the phenomena which are peculiar to individual crystals; and I have not ventured to adopt any theory of their formation.

I cannot, however, conclude this paper without noticing the happy application which Dr. THOMAS YOUNG has made of his beautiful law of interference to the explanation of

this class of colours. The various phenomena of thick and thin plates; the colours of inflected light; the hues of the supernumerary rainbow; the fringes which I observed with two inclined plates of glass of equal thickness; the colours noticed by Mr. NICHOLSON and Mr. KNOX, with plates of unequal thickness; the communicable colours of mother of pearl and striated surfaces; and the colours which I have discovered by the successive reflection of polarised light between metallic plates, and surfaces that produce total reflection, are all referable to this simple law. But while we thus admire the wide range of phenomena which the law of interference embraces, it is necessary also to state, that its application to the colours of polarised light is still attended with some difficulties, and that there are other phenomena of complementary colours which I have lately observed, that resist this method of classification. These, however, are merely difficulties in the application of the law, and not objections to its generality; and I have no doubt that Dr. YOUNG will succeed in referring them to the same cause, and will thus add to the honour which already belongs to him, of having generalised a long train of perplexing and important phenomena.*

I have the honour to be, &c. &c.

DAVID BREWSTER.

To the Right Hon.

Sir Joseph Banks, Bart. &c. &c. &c.

* Having communicated to Dr. YOUNG the contents of the preceding paper, he has requested me to subjoin to it the following letter:

My Dear Sir,

Your experiments, on the colours afforded by crystals having two optical axes, appear to establish a very important result in the theory of light; for

Fig. 1.

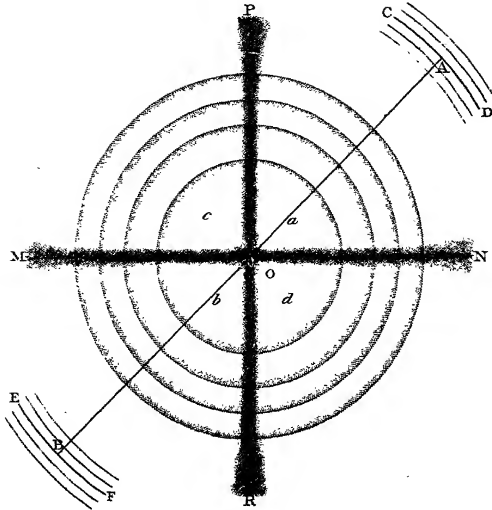


Fig. 2.

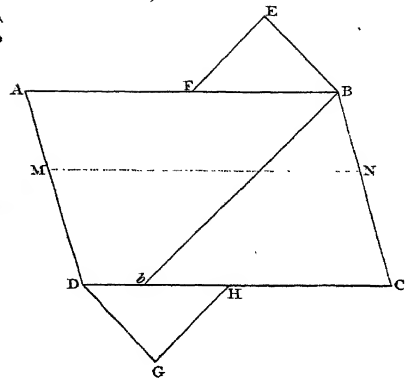


Fig. 3.

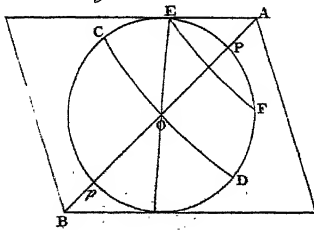


Fig. 6.

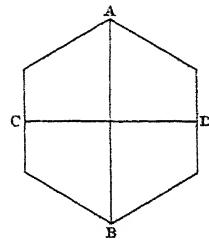


Fig. 4.

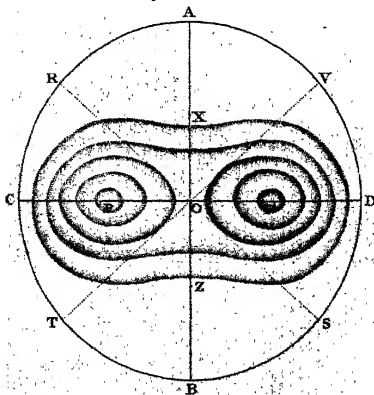
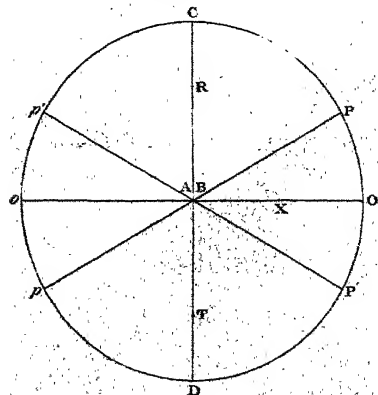


Fig. 5.



supposing them to be perfectly represented by your general law, it will follow that the tint exhibited depends not on the difference of refractive densities in the direction of the ray transmitted, but on the greatest difference of refractive densities in directions perpendicular to that of the ray. These two conditions lead to the same result, where the effect of one axis only is considered, but they vary materially where two axes are supposed to be combined; and I do not immediately perceive by what modification it will be possible to accommodate the laws of interference to these experiments. There can be little doubt that the direction of the polarisation, in such cases, must be determined by that of the greatest and least of the refractive densities in question†; and it seems to be very possible to apply your mode of calculation to many other phenomena, in which the polarising powers of different crystals are combined.

Believe me, dear Sir,

Your faithful and obedient servant,

THOMAS YOUNG."

† This supposition of Dr. YOUNG's is quite correct. In another paper, which will soon be submitted to the Royal Society, I have given a general method of finding the direction of the polarisation for any combination of axes.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCXVIII.

METEOROLOGICAL JOURNAL

for January, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	inches.	Points.	Str.	
Jan. 1	8	0	43	51	29.56	S	1,2	Fair.
	2	0	47	50	29.45	S	1,2	Rain.
2	8	0	42	50	29.41	S	1	Fine.
	2	0	46	55	29.50	S	1	Fine.
3	8	0	37	50	29.57	W	1	Fine, much wind in the night.
	2	0	42	52	29.62	W	1	Fine.
4	8	0	47	50	29.27	SW	2,3	Rain.
	2	0	51	54	29.27	SW	2	Cloudy.
5	8	0	40	50	29.82	NW	1,2	Fine.
	2	0	45	51	29.79	W	1	Rain. [in the night.
6	8	0	42	48	29.57	S	2	Cloudy, much wind and rain
	2	0	43	52	29.55	W	1	Cloudy.
7	8	0	35	47	30.27	W	1	Cloudy.
	2	0	40	51	30.43	N	1	Fine.
8	8	0	31	48	30.44	NW	1	Thick Fog.
	2	0	41	51	30.44	NW	1	Cloudy.
9	8	0	35	46	30.56	SSE	1	Fine.
	2	0	38	50	30.58	S	1	Foggy.
10	8	0	29	47	30.61	W	1	Foggy.
	2	0	28	46	30.55	W	1	Cloudy.
11	8	0	30	44	30.43	W	1	Cloudy thick weather.
	2	0	34	37	30.36	NNW	1	Cloudy.
12	8	0	32	43	30.15	W	1	Cloudy.
	2	0	38	44	30.04	NNW	1	Cloudy.
13	8	0	34	42	29.78	W	1	Cloudy and hazy.
	2	0	38	46	29.64	SW	1	Cloudy.
14	8	0	35	43	29.54	NW	1	Fine.
	2	0	38	48	29.57	W	1	Fine.
15	8	0	33	44	28.95	E	1	Rain, snow in the night.
	2	0	34	46	28.89	S	1	Snow.
16	8	0	31	42	29.34	N	1	Cloudy.
	2	0	37	48	29.00	S	2	Rain.

Rain this Month 2, 15, 14 Inches.

METEOROLOGICAL JOURNAL

for January, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Jan. 17	8	0	41	46	28.88	S	1	Cloudy.
	2	0	44	48	28.89	S	1	Cloudy.
18	8	0	43	48	29.06	S	1	Fine.
	2	0	44	52	29.09	S	1	Cloudy.
19	8	0	41	48	29.07	E	1	Cloudy.
	2	0	43	48	28.86	E	1	Cloudy.
20	8	0	47	47	28.80	SSE	2,3	Fine.
	2	0	48	51	28.82	S	3	Cloudy.
21	8	0	37	48	29.49	W	1	Cloudy.
	2	0	42	54	29.70	W	1	Fine.
22	8	0	42	49	29.77	W	2	Rain.
	2	0	47	53	29.86	W	1,2	Cloudy.
23	8	0	41	51	29.88	W	1	Cloudy.
	2	0	53	56	30.02	W	1	Cloudy.
24	8	0	48	54	30.20	S	1	Cloudy.
	2	0	49	57	30.24	W	1	Cloudy.
25	8	0	48	54	30.31	W	1	Cloudy.
	2	0	49	56	30.20	SW	1	Cloudy.
26	8	0	44	53	30.25	S	1	Cloudy.
	2	0	45	53	30.19	W	1	Cloudy.
27	8	0	47	52	30.31	W	1	Cloudy, thick weather.
	2	0	49	54	30.35	E	1	Cloudy.
28	8	0	43	52	30.49	W	1	Dark and Cloudy.
	2	0	45	56	30.34	S	1	Cloudy.
29	8	0	44	51	30.30	W	1	Cloudy.
	2	0	47	57	30.33	NW	1	Cloudy.
30	8	0	43	52	30.33	W	1	Cloudy.
	2	0	50	56	30.28	E	1	Cloudy.
31	8	0	43	53	30.43	N	1	Cloudy.
	2	0	49	57	30.47	N	1	Cloudy.

Rain this Month 2,154 Inches.

METEOROLOGICAL JOURNAL

for February, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H. M.	o	o	Inches.		Points.	Str.	
Feb.	1	8 0	40	52	30.53	N	1	Cloudy and hazy.
		2 0	50	57	30.53	W	1	Cloudy.
	2	8 0	42	52	30.50	W	1	Cloudy.
		2 0	44	52	30.44	W	1	Cloudy.
	3	8 0	38	49	30.35	W	1	Cloudy.
		2 0	42	53	30.27	NW	1	Cloudy.
	4	8 0	40	50	30.01	SW	1	Cloudy.
		2 0	43	55	29.76	SW	1	Cloudy.
	5	8 0	39	49	29.73	NW	1	Fine.
		2 0	45	54	29.90	NW	1	Cloudy.
	6	8 0	49	51	30.01	W	1	Cloudy.
		2 0	50	57	30.06	W	1	Cloudy.
	7	8 0	45	53	30.17	W	1	Hazy.
		2 0	52	58	30.26	WNW	1,2	Cloudy.
	8	8 0	46	54	30.28	W	1	Cloudy.
		2 0	46	55	30.33	W	1	Cloudy.
	9	8 0	49	54	30.35	W	1	Cloudy.
		2 0	49	52	30.36	W	1	Cloudy.
	10	8 0	48	52	30.18	W	1	Cloudy.
		2 0	49	55	30.07	W	1,2	Cloudy.
	11	8 0	42	53	29.84	N	1	Rain.
		2 0	43	56	30.00	N	1	Fine.
	12	8 0	40	51	29.47	W	1	Cloudy.
		2 0	45	53	29.61	N	1	Cloudy.
	13	8 0	40	50	29.85	W	1	Rain.
		2 0	50	57	29.79	W	1	Rain.
	14	8 0	41	53	29.50	W	1	Cloudy, rainy, and stormy
		2 0	43	57	29.70	N	1	Fine.

Rain this Month 0.875 Inches.

[night.]

METEOROLOGICAL JOURNAL

for February, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Feb. 15	8	0	40	53	29.53	W	1	Rain.
	2	0	50	56	29.52	NW	1	Fine.
16	8	0	41	51	29.70	NNW	1	Fine.
	2	0	49	52	29.83	NW	1	Fine.
17	8	0	46	50	29.96	W	1	Rain.
	2	0	53	54	29.97	W	1	Fine.
18	8	0	48	53	30.11	W	1	Cloudy.
	2	0	51	56	30.01	W	1	Cloudy.
19	8	0	42	53	30.18	NNW	1	Hazy.
	2	0	47	58	30.24	S	1	Fair.
20	8	0	44	52	29.85	S	1.2	Cloudy, hard gale of wind in
	2	0	49	57	29.51	S	1	Cloudy. [the night.
21	8	0	37	52	29.56	W	1	Fine.
	2	0	44	55	29.52	Wby N	1	Fine.
22	8	0	40	52	29.62	N	1	Fine.
	2	0	46	57	29.81	NW	1	Cloudy.
23	8	0	40	52	29.98	W	1	Cloudy.
	2	0	49	52	29.82	S	1	Cloudy.
24	8	0	43	51	29.77	W	1	Cloudy.
	2	0	47	55	29.91	W	1	Cloudy.
25	8	0	42	52	30.01	W	1	Fine.
	2	0	50	57	29.98	W	1	Cloudy.
26	8	0	42	53	29.73	N	1	Fair.
	2	0	48	57	29.88	WNW	1	Cloudy.
27	8	0	47	53	29.60	W	1	Fine.
	2	0	50	55	29.66	NW	1.2	Fine.
28	8	0	46	54	29.88	W	1	Cloudy.
	2	0	48	60	29.87	W	1	Fair.

Rain this Month 0.875 Inches.

METEOROLOGICAL JOURNAL

for March, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Mar. 1	7	0	47	56	29.77	W	1	Cloudy.
	2	0	52	59	29.57	SW	1	Cloudy.
2	7	0	39	53	29.74	W	1	Cloudy.
	2	0	48	55	29.66	W	1	Fine.
3	7	0	39	51	29.44	SW	1	Fine, a stormy night.
	2	0	43	54	29.06	W	1,2	Rain.
4	7	0	40	51	29.22	W	1	Fine.
	2	0	42	56	29.21	W	1	Fine.
5	7	0	36	50	29.27	E	1	Fair.
	2	0	48	56	29.35	NW	1	Fair.
6	7	0	40	51	28.87	W	1	Rain.
	2	0	44	56	29.07	NW	1	Fine.
7	7	0	36	50	29.26	W	1	Fine.
	2	0	47	56	29.35	NW	1	Fine.
8	7	0	38	51	29.07	W	1	Rain.
	2	0	49	56	29.24	W	1	Fine, a fall of snow at 10 a. m.
9	7	0	37	49	29.46	W	1	Cloudy.
	2	0	47	53	29.61	W	2	Fine.
10	7	0	36	48	29.98	W	1	Fair.
	2	0	52	55	30.06	WNW	1	Fine.
11	7	0	37	49	30.17	W	1	Fine.
	2	0	48	53	30.17	W	1	Cloudy.
12	7	0	44	51	30.02	S	1	Fine.
	2	0	51	56	29.94	W	1	Cloudy.
13	7	0	48	54	29.98	N	1	Cloudy and hazy.
	2	0	53	57	30.01	WNW	1	Cloudy.
14	7	0	45	54	30.22	E	1	Fine.
	2	0	55	61	30.24	E	1	Fine.
15	7	0	40	54	30.28	N	1	Cloudy.
	2	0	49	58	30.29	N	1	Cloudy.
16	7	0	39	52	30.24	SSE	1	Cloudy.
	2	0	46	57	30.20	E	1	Fine.

METEOROLOGICAL JOURNAL

for March, 1817.

1817	Time.	Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H. M.	°	°	Inches.	Points.	Str.	
Mar. 17	7 0	38	51	30.27	N	1	Cloudy.
	2 0	45	55	30.31	E	1	Fine.
18	7 0	37	51	30.28	WSW	1	Fine.
	2 0	50	56	30.15	SW	1	Fine.
19	7 0	45	53	29.92	W	1	Fine.
	2 0	51	57	29.90	N	1	Fine.
20	7 0	34	51	29.79	NW	1	Fine.
	2 0	38	53	29.85	N	1	Cloudy.
21	7 0	30	47	29.93	N	1	Fine.
	2 0	39	54	29.94	N	1	Fine.
22	7 0	30	48	29.94	N	1	Hazy.
	2 0	40	54	29.97	N	1	Fine.
23	7 0	31	50	30.05	E	1	Fine.
	2 0	44	55	30.03	S	1	Fine.
24	7 0	44	49	29.95	SW	1	Cloudy.
	2 0	49	54	29.87	SW	1	Cloudy.
25	7 0	46	51	29.79	W	1	Rain.
	2 0	50	56	29.89	NE	1	Fine.
26	7 0	44	52	29.95	W	1	Cloudy.
	2 0	50	56	29.84	W	1	Cloudy.
27	7 0	39	52	30.03	W	1	Fine.
	2 0	46	58	30.13	W	1	Fine.
28	7 0	39	51	30.00	S	1	Cloudy.
	2 0	48	53	29.84	W	1	Cloudy.
29	7 0	45	50	29.90	W	1	Cloudy.
	2 0	50	54	29.99	W	1	Cloudy.
30	7 0	48	55	30.05	W	1	Fair.
	2 0	56	58	30.10	N	1	Fine.
31	7 0	44	53	30.25	NW	1	Cloudy.
	2 0	52	59	30.50	N	1	Fine.

Rain this Month 1.057 Inches.

METEOROLOGICAL JOURNAL

for April, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.	
	H.	M.	°	°	Inches.	Points.	Str.		
Apr.	1	7	0	45	53	30,56	S	1	Fair.
		2	0	53	63	30,54	E	1	Fair.
	2	7	0	43	55	30,45	E	1	Thick and foggy.
		2	0	53	63	30,38	E	1	Fair.
	3	7	0	43	57	30,35	N	1	Fine, but rather hazy.
		2	0	54	62	30,35	N	1	Fine.
	4	7	0	45	57	30,42	N	1	Fine.
		2	0	52	64	30,40	E	1	Fine.
	5	7	0	43	56	30,38	E	1	Dark and hazy.
		2	0	50	62	30,37	N	1	Fine.
	6	7	0	40	54	30,38	N	1	Fair.
		2	0	47	55	30,43	N	1	Cloudy.
	7	7	0	44	53	30,49	N	1	Cloudy.
		2	0	50	61	30,47	E	1	Fine.
	8	7	0	40	53	30,27	W	1	Hazy.
		2	0	53	62	30,04	W	1	Fine.
	9	7	0	44	54	29,95	SW	1	Fine, rain in the night.
		2	0	50	57	30,04	NE	1	Cloudy.
	10	7	0	36	52	30,03	N	1	Fair.
		2	0	40	56	30,13	N	1	Fine.
	11	7	0	34	50	30,30	N	1	Cloudy.
		2	0	44	55	30,30	N	1	Cloudy.
	12	7	0	42	52	30,12	W	1	Cloudy.
		2	0	59	55	30,06	N	1	Cloudy.
	13	7	0	46	53	30,08	NW	1	Cloudy.
		2	0	52	53	30,12	N	1	Cloudy.
	14	7	0	48	52	30,09	N	1	Fine, rather hazy.
		2	0	51	59	30,06	N	1	Fine.
	15	7	0	53	55	29,99	N	1	Cloudy.
		2	0	58	60	29,96	N	1	Fine.

Rain this Month 0,120 Inches.

METEOROLOGICAL JOURNAL

for April, 1817.

1817	Time.	Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H. M.	°	°	Inches.	Points.	Str.	
Apr. 16	7 0	52	56	29.75	N	1	Cloudy.
	2 0	50	59	29.95	N	1,2	Fine.
17	7 0	40	52	30.17	N	1,2	Fine.
	2 0	45	55	30.25	N	1	Cloudy.
18	7 0	40	52	30.39	N	1	Cloudy.
	2 0	49	57	30.43	N	1	Cloudy.
19	7 0	40	53	30.45	NW	1	Cloudy.
	2 0	52	58	30.43	NE	1	Fine.
20	7 0	48	55	30.43	N	1	Fair.
	2 0	55	60	30.44	N	1	Fine.
21	7 0	45	54	30.43	N	1	Fine.
	2 0	48	62	30.38	N	1	Fine.
22	7 0	44	55	30.36	NW	1	Cloudy.
	2 0	52	63	30.31	NE	1	Fine.
23	7 0	43	55	30.27	NE	1	Cloudy.
	2 0	49	57	30.24	E	1	Rain.
24	7 0	41	53	30.28	SSE	1	Cloudy.
	2 0	53	58	30.27	N	1	Fine.
25	7 0	42	53	30.32	N	1	Cloudy.
	2 0	42	56	30.27	N	1	Cloudy.
26	7 0	42	53	30.13	SE	1	Cloudy.
	2 0	48	56	30.09	N	1	Cloudy.
27	7 0	43	54	29.99	SE	1,2	Cloudy.
	2 0	51	55	30.07	N	1	Fair.
28	7 0	43	50	30.18	W	1	Cloudy.
	2 0	53	54	30.14	W	1	Cloudy.
29	7 0	46	48	30.01	W	1	Cloudy.
	2 0	53	57	29.89	W	1	Cloudy.
30	7 0	44	54	29.76	W	1	Cloudy.
	2 0	48	59	29.78	N	1	Fine.

Rain this Month 0.120 Inches.

METEOROLOGICAL JOURNAL

for May, 1817.

1817.	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
May 1	7	0	45	54	29.87	N	1	Cloudy.
	2	0	49	57	29.92	N	1	Cloudy.
2	7	0	42	54	30.00	N	1	Cloudy.
	2	0	53	60	30.00	N	1	Cloudy.
3	7	0	44	55	29.95	WNW	1	Cloudy.
	2	0	55	60	29.85	N	1	Cloudy.
4	7	0	50	56	29.84	W	1	Cloudy.
	2	0	57	59	29.92	W	1	Fine.
5	7	0	48	54	30.19	S	1	Fine.
	2	0	58	62	30.07	W	1	Fair.
6	7	0	50	57	30.14	W	1	Fine.
	2	0	58	61	30.19	N	1	Fine.
7	7	0	48	56	30.29	E	1	Fine.
	2	0	55	61	30.14	E	1	Fine.
8	7	0	52	57	29.88	NE	1	Hazy.
	2	0	63	62	29.81	E	1	Cloudy.
9	7	0	48	58	29.81	E	1	Cloudy.
	2	0	52	61	29.76	NE	1	Cloudy.
10	7	0	49	57	29.60	S	1	Cloudy.
	2	0	58	61	29.49	S	1	Fine.
11	7	0	47	57	29.50	NW	1	Hazy.
	2	0	57	60	29.54	NW	1	Fine.
12	7	0	48	55	29.34	W	1	Cloudy.
	2	0	58	62	29.42	E	1	Fine.
13	7	0	46	55	29.71	W	1	Fine.
	2	0	51	58	29.78	SSW	1	Fine.
14	7	0	42	54	29.69	SE	1	Rain.
	2	0	50	56	29.57	N	1	Fine.
15	7	0	47	55	29.78	W	1	Cloudy.
	2	0	57	58	29.88	N	1	Fine.
16	7	0	49	57	29.98	SW	1	Fair.
	2	0	64	64	29.94	E	1	Fine.

Rain this Month 1.698 inches.

METEOROLOGICAL JOURNAL

for May, 1817.

1817	Time.	Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H. M.	°	°	Inches.	Points.	Str.	
May 17	7 0	51	57	29.89	W	1	Showery.
	2 0	61	62	29.83	W	1	Cloudy.
18	7 0	54	59	29.69	E	1	Cloudy.
	2 0	65	62	29.55	E	1	Rain.
19	7 0	50	58	29.52	NE	1	Cloudy.
	2 0	53	60	29.54	N	1	Rain.
20	7 0	45	57	29.51	N	1	Rain.
	2 0	52	59	29.51	N	1	Cloudy.
21	7 0	47	57	29.44	NW	1	Rain.
	2 0	42	59	29.48	W	1.2	Rain.
22	7 0	44	55	29.48	W	1	Cloudy.
	2 0	50	59	29.49	W	1	Fair.
23	7 0	46	56	29.52	SW	1	Cloudy.
	2 0	57	62	29.51	SW	1	Cloudy.
24	7 0	46	55	29.52	W	1	Fine.
	2 0	58	59	29.52	W	1	Fine.
25	7 0	42	57	29.37	E	1	Rain.
	2 0	52	55	29.28	W	1	Rain.
26	7 0	49	56	29.23	E	1	Cloudy.
	2 0	59	60	29.27	E	1	Cloudy.
27	7 0	52	57	29.54	E	1	Cloudy.
	2 0	64	60	29.52	ESE	1	Fair.
28	7 0	49	56	29.66	N	1	Fair.
	2 0	54	60	29.71	SW	1	Cloudy.
29	7 0	50	58	29.65	NE	1	Cloudy.
	2 0	50	60	29.69	N	1.2	Cloudy.
30	7 0	47	56	29.87	N	1	Cloudy.
	2 0	51	58	29.91	N	1	Cloudy.
31	7 0	45	54	29.95	N	1	Fine.
	2 0	57	58	29.87	N	1	Cloudy.

Rain this Month 1.698 Inches.

METEOROLOGICAL JOURNAL

for June, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.	
	H.	M.	°	°	Inches.	Points.	Str.		
June	1	7	0	47	55	29,78	NW	1	Cloudy.
		2	0	57	56	29,79	N	1	Cloudy.
	2	7	0	53	55	29,78	SW	1	Fine.
		2	0	56	57	29,75	W	1	Cloudy.
	3	7	0	53	55	29,70	W	1	Cloudy.
		2	0	60	61	29,74	SW	1.2	Fine.
	4	7	0	48	58	29,52	W	1.2	Rain.
		2	0	62	61	29,76	N	2	Fine.
	5	7	0	53	57	30,10	W	1	Cloudy.
		2	0	59	60	30,11	SW	1	Fair.
	6	7	0	54	59	30,03	W	1	Cloudy.
		2	0	66	64	30,03	W	1	Fine.
	7	7	0	59	61	29,95	W	1	Cloudy.
		2	0	67	67	29,87	S	1	Fine.
	8	7	0	57	62	29,74	W	1	Cloudy.
		2	0	62	65	29,78	NW	1	Cloudy.
	9	7	0	55	60	29,88	W	1	Cloudy.
		2	0	54	62	29,82	SW	1	Rain.
	10	7	0	56	60	29,75	W	1.2	Cloudy.
		2	0	64	63	29,84	WNW	1	Fine.
	11	7	0	52	59	30,06	W	1	Fine.
		2	0	62	62	30,02	W	1	Fine.
	12	7	0	55	60	29,80	S	1	Cloudy, rain in the night.
		2	0	56	61	29,68	SW	1	Cloudy.
	13	7	0	53	59	29,65	E	1	Cloudy.
		2	0	60	61	29,38	S	2	Rain.
	14	7	0	53	59	29,42	W	2	Fine.
		2	0	58	60	29,61	W	1.2	Fine.
	15	7	0	53	58	30,12	Nby W	1	Fine.
		2	0	60	59	30,23	NW	1	Fine.

Rain this Month 1,860 Inches.

METEOROLOGICAL JOURNAL

for June, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H. M.	°	°	Inches.		Points.	Str.	
June 16	7	0	52	58	30.32	WSW	1	Hazy.
	2	0	65	62	30.28	E	1	Fine.
17	7	0	56	59	30.14	E	1	Fine.
	2	0	65	65	30.00	E	1	Fine.
18	7	0	61	60	29.75	E	1	Cloudy and hazy.
	2	0	71	69	29.69	E	1	Fine.
19	7	0	67	63	29.70	SW	1	Fine.
	2	0	77	70	29.72	SW	1	Fine, with showers.
20	7	0	68	66	29.79	E	1	Hazy.
	2	0	79	75	29.79	SE	1	Fair.
21	7	0	69	69	29.87	N	1	Fine.
	2	0	81	77	29.89	SE	1	Fine.
22	7	0	70	71	30.06	NNW	1	Fine.
	2	0	80	77	30.05	NNE	1	Fair.
23	7	0	68	70	30.01	N	1	Fair.
	2	0	78	76	29.96	N	1	Fine.
24	7	0	67	70	29.93	NNW	1	Thick and cloudy.
	2	0	77	77	29.93	WNW	1	Fine.
25	7	0	67	70	29.97	W	1	Fine.
	2	0	74	77	29.96	W	1	Fine.
26	7	0	63	67	29.87	SW	1	Cloudy.
	2	0	74	74	29.79	SW	1	Fine.
27	7	0	69	70	29.64	E	1	Thick and hazy.
	2	0	75	74	29.59	E	1	Fine.
28	7	0	64	66	29.70	W	1	Cloudy.
	2	0	70	71	29.79	W	1	Fine.
29	7	0	59	67	29.96	S	1	Cloudy.
	2	0	68	69	29.91	SSE	1	Fine.
30	7	0	61	66	29.71	W	1	Cloudy.
	2	0	67	69	29.75	NW	1	Fine.

Rain this Month 1.867 Inches.

METEOROLOGICAL JOURNAL

for July, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.	
	H.	M.	°	°	Inches.	Points.	Str.		
July	1	7	0	58	65	29,80	E	1	Cloudy.
		2	0	59	66	29,58	S	2	Rain.
	2	7	0	57	64	29,61	W	1, 2	Cloudy.
		2	0	66	67	29,83	W	1	Cloudy.
	3	7	0	57	64	29,94	W	1	Cloudy.
		2	0	62	65	29,88	S	1	Rain.
	4	7	0	56	63	29,65	W	1	Cloudy.
		2	0	64	65	29,64	W	1	Fine.
	5	7	0	57	60	29,55	W by N	1	Cloudy.
		2	0	64	64	29,60	NW	1	Cloudy.
	6	7	0	57	62	29,73	S	1	Cloudy.
		2	0	66	66	29,72	S	1	Fair.
	7	7	0	57	62	29,78	W	1	Fine.
		2	0	68	65	29,77	W	1	Cloudy.
	8	7	0	58	63	29,84	W	1	Lazy.
		2	0	66	66	29,86	W	1	Cloudy.
	9	7	0	60	61	29,88	NNW	1	Cloudy.
		2	0	68	66	29,88	SW	1	Cloudy.
	10	7	0	60	63	29,84	SSW	1	Fine.
		2	0	70	66	29,81	SSW	1	Fine.
	11	7	0	62	64	29,79	SW	1	Cloudy.
		2	0	67	67	29,79	SW	1	Cloudy.
	12	7	0	62	64	29,83	NW	1	Cloudy.
		2	0	63	65	29,94	SW	1	Cloudy.
	13	7	0	58	64	29,99	SW	1	Fine.
		2	0	64	64	29,94	SW	1	Cloudy.
	14	7	0	55	59	29,74	W	1	Cloudy, rain in the night.
		2	0	62	64	29,61	WNW	1	Cloudy.
	15	7	0	55	61	29,27	SW	1	Rain.
		2	0	61	65	29,17	WNW	1	Cloudy.
	16	7	0	54	60	29,62	N	1	Fine.
		2	0	61	62	29,71	NNW	1	Fine.

METEOROLOGICAL JOURNAL

for July, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
July 17	7	0	56	60	29,85	N	1	Fine.
	2	0	63	63	29,88	NW	1	Fine.
18	7	0	57	61	29,81	WNW	1	Cloudy.
	2	0	63	60	29,82	WNW	1	Fair.
19	7	0	56	60	29,95	W by N	1	Fine, rather hazy.
	2	0	62	61	29,97	W	1	Cloudy.
20	7	0	57	58	29,98	W	1	Fine.
	2	0	62	60	29,99	WNW	1	Fine.
21	7	0	58	60	29,93	SW	1	Cloudy.
	2	0	66	64	29,88	SW	1,2	Cloudy.
22	7	0	59	62	29,78	SW	1	Rain.
	2	0	65	64	29,81	SW	1	Cloudy.
23	7	0	60	62	29,89	NW	1	Hazy.
	2	0	63	64	29,92	N	1	Cloudy, thunder in the evening.
24	7	0	58	62	30,03	SW	1	Hazy.
	2	0	67	65	30,04	NW	1	Fair.
25	7	0	60	62	30,01	S	2	Fine.
	2	0	66	68	30,00	S	1	Fine.
26	7	0	59	63	29,94	S	1	Cloudy.
	2	0	60	63	29,80	NNW	1	Cloudy.
27	7	0	57	62	29,66	W by N	1	Fine.
	2	0	65	65	29,80	W	1	Fine.
28	7	0	57	57	29,84	W	1	Cloudy.
	2	0	64	64	29,89	SW	1	Fine.
29	7	0	59	61	30,05	W	1	Fine.
	2	0	61	63	29,98	W	1	Cloudy.
30	7	0	58	61	29,80	W	1	Cloudy.
	2	0	60	63	29,80	NNW	1	Cloudy.
31	7	0	57	61	29,77	W	1	Cloudy.
	2	0	60	63	29,83	W	1	Rain.

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for August, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Aug. 1	7	0	53	61	29,79	W	1	Fair.
	2	0	64	65	29,82	NW	1	Fine.
2	7	0	54	60	29,93	W	1	Fine.
	2	0	65	67	29,94	NW	1	Cloudy.
3	7	0	57	62	29,78	SW	2	Cloudy.
	2	0	55	64	29,73	NW	1	Fine.
4	7	0	57	61	29,80	W	2	Cloudy.
	2	0	67	66	29,76	NNW	1.2	Fine.
5	7	0	58	62	29,96	NNE	1	Cloudy.
	2	0	68	63	30,06	N	1	Fine.
6	7	0	59	62	30,08	SW	1	Fine.
	2	0	67	68	30,06	SW	1	Fine.
7	7	0	58	63	29,96	E	1	Hazy.
	2	0	68	66	29,85	SW	1	Fine.
8	7	0	57	64	29,57	SW	2	Rain.
	2	0	67	65	29,56	W	1.2	Cloudy.
9	7	0	57	63	29,72	W	1.2	Fine.
	2	0	65	67	29,77	W	1	Cloudy.
10	7	0	58	63	29,86	NW	1	Cloudy.
	2	0	63	65	29,85	NW	1	Cloudy.
11	7	0	53	62	29,86	NW	1	Fine.
	4	0	63	64	29,69	SE	1	Cloudy.
12	7	0	55	62	29,42	W	1	Fine.
	4	0	60	63	29,42	S	1	Cloudy.
13	7	0	58	61	29,25	S	1	Cloudy.
	4	0	60	66	29,58	SW	1	Fine.
14	7	0	60	62	29,57	W	1	Cloudy.
	4	0	68	69	29,76	W	1	Rain.
15	7	0	62	61	29,78	N	1	Cloudy.
	4	0	62	67	29,77	NW	1	Cloudy.
16	7	0	61	61	29,76	S	1	Cloudy.
	4	0	59	64	29,79	SSW	1	Cloudy.

Rain this Month 1,458 Inches.

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for August, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Aug. 17	7	0	60	60	29.75	NW	1	Cloudy.
	4	0	61	66	29.77	W	1	Cloudy.
18	7	0	60	65	29.76	NE	1	Rain.
	4	0	55	60	29.71	E	1	Cloudy.
19	7	0	54	63	29.90	NW	1	Rain.
	4	0	51	65	29.72	N	1	Fair.
20	7	0	56	67	29.81	N	1	Cloudy.
	4	0	61	63	29.84	W	1	Cloudy.
21	7	0	59	65	29.68	N	1	Cloudy.
	4	0	59	68	29.67	N	1	Rain.
22	7	0	60	61	29.60	W	1	Rain.
	4	0	60	68	29.81	N	1	Rain.
23	7	0	65	67	30.01	E	1	Cloudy.
	4	0	64	66	30.07	N	1	Fine.
24	7	0	63	64	29.99	W	1	Fine.
	4	0	62	61	29.70	W	1	Fine.
25	7	0	60	63	29.61	N	1	Fine.
	4	0	60	61	29.00	S	1	Rain.
26	7	0	59	63	29.16	N	1	Rain.
	4	0	53	62	29.10	E	1	Fair.
27	7	0	53	60	29.29	W	1	Rain.
	4	0	50	60	29.30	E	1	Rain.
28	7	0	56	69	29.60	W	1	Rain.
	4	0	57	68	29.63	E	1	Fair.
29	7	0	54	67	29.64	W	1	Fine.
	4	0	55	68	29.71	N	1	Rain.
30	7	0	59	64	29.79	NE	1	Rain.
	4	0	62	65	29.85	N	1	Fine.
31	7	0	60	68	29.81	E	1	Fine.
	4	0	56	67	29.80	SE	1	Fine.

Rain this Month 1.458 Inches.

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for September, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	o	o	Inches.	Points.	Str.	
Sept. 1	7	0	50	53	30.00	N	1	Fine.
	4	0	59	61	30.01	E	1	Fine.
2	7	0	55	56	29.95	N	1	Fine.
	4	0	59	61	29.97	N	1	Fine.
3	7	0	60	63	29.84	E	1	Fine.
	4	0	61	67	29.87	NNE	1	Fine.
4	7	0	67	63	30.01	N	1	Fine.
	4	0	69	62	30.03	NE	1	Fine.
5	7	0	55	60	30.11	N	1	Fine.
	4	0	59	63	30.12	NNE	1	Fine.
6	7	0	55	61	30.01	N	1	Fine.
	4	0	60	67	30.07	NE	1	Fine.
7	7	0	56	61	30.09	E	1	Fair.
	4	0	60	63	30.06	SSE	1	Fair.
8	7	0	56	62	29.99	S	1	Fair.
	4	0	61	63	29.97	S	1	Fair.
9	7	0	58	59	30.00	E	1	Fair.
	4	0	57	60	29.99	E	1	Fair.
10	7	0	59	61	29.97	E	1	Fair.
	4	0	54	59	29.95	W	1	Fair.
11	7	0	57	59	30.01	NW	1	Fine.
	4	0	56	55	30.00	N	1	Cloudy.
12	7	0	57	60	29.96	NNW	1	Cloudy.
	4	0	59	60	29.95	N	1	Cloudy.
13	7	0	54	59	29.94	N	1	Fine.
	4	0	55	65	29.96	N	1	Fine.
14	7	0	54	60	29.87	E	1	Rain.
	4	0	61	67	29.97	NE	1	Rain.
15	7	0	60	63	30.01	N	1	Rain.
	4	0	62	65	30.11	NW	1	Rain.

Rain this Month 0.300 Inches.

METEOROLOGICAL JOURNAL

for September, 1817.

1817	Time.	Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H. M.	°	°	Inches.	Points.	Str.	
Sep. 16	7 0	64	69	30,01	N	1,2	Cloudy.
	4 0	56	60	30,10	N	2	Cloudy.
17	7 0	56	59	29,93	E	1	Fine.
	4 0	61	63	29,90	NE	1	Cloudy.
18	7 0	56	60	29,87	E	1	Cloudy.
	4 0	59	63	29,86	E	1	Cloudy.
19	7 0	58	60	29,89	NW	1	Cloudy.
	4 0	56	60	29,97	NW	1	Cloudy.
20	7 0	55	61	30,00	N	1	Fine.
	4 0	57	63	30,01	N	1	Fine.
21	7 0	55	60	29,97	N	1	Fine.
	4 0	53	61	29,95	E	1	Fine.
22	7 0	49	60	29,84	E	1	Fine.
	4 0	53	61	29,87	E	1	Fine.
23	7 0	51	66	29,83	E	1	Fine.
	4 0	56	63	29,85	NE	1	Fine.
24	7 0	55	59	29,97	N	1	Fine.
	4 0	57	58	29,86	NW	1	Cloudy.
25	7 0	60	61	29,53	N	1	Fine.
	4 0	58	60	29,55	N	1	Fine.
26	7 0	57	60	29,22	E	1	Fine.
	4 0	51	59	29,27	W	1	Fine.
27	7 0	59	54	30,00	N	1	Fine.
	4 0	51	55	30,01	NE	1	Fair.
28	7 0	60	66	30,11	E	1	Fine.
	4 0	63	65	30,19	S	1	Fine.
29	7 0	56	54	30,08	E	1	Fair.
	4 0	57	56	30,06	E	1	Fine.
30	7 0	58	55	29,94	S	1	Fine.
	4 0	57	60	30,00	S	1	Fine.

Rain this Month 0,300 Inches.

METEOROLOGICAL JOURNAL

for October, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Oct. 1	7	0	59	65	29.08	SE	1	Fair.
	4	0	48	54	29.08	W	1	Fine.
2	7	0	54	55	29.18	N	1	Fine.
	4	0	53	50	30.10	NE	1	Fine.
3	7	0	50	47	30.11	E	1	Fine.
	4	0	52	48	30.09	NE	1	Fine.
4	7	0	53	53	30.25	N	1	Fine.
	4	0	54	53	30.13	N	1	Fine.
5	7	0	57	59	30.01	E	1	Fine.
	4	0	53	56	30.11	W	1	Fine.
6	7	0	50	52	30.30	E	1	Fine.
	4	0	52	54	30.29	E	1	Fine.
7	7	0	53	52	30.16	W	1	Fine.
	4	0	55	53	30.20	W	1	Fine.
8	7	0	52	53	30.14	E	1	Fine.
	4	0	52	54	30.10	S	1	Fine.
9	7	0	52	53	30.02	SE	1	Fine.
	4	0	53	55	30.03	SW	1	Fine.
10	7	0	52	54	29.92	S	1	Cloudy.
	4	0	53	54	29.93	SW	1	Cloudy.
11	7	0	53	56	30.01	SW	1	Cloudy.
	4	0	49	55	30.10	SE	1	Cloudy.
12	8	0	47	52	30.04	SE	1	Fine.
	2	0	50	55	30.05	SW	1	Fine.
13	8	0	43	51	30.20	NNE	1	Cloudy.
	2	0	52	56	30.27	N	1	Rain.
14	8	0	39	52	30.28	W	1	Cloudy.
	2	0	49	56	30.20	N	1	Cloudy.
15	8	0	48	54	30.08	NNE	1	Cloudy.
	2	0	48	56	29.99	NE	1	Rain.
16	8	0	45	52	29.85	NE	1	Rain.
	2	0	46	56	29.95	E	1	Cloudy.

Rain this Month 0,553 Inches.

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for October, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Oct. 17	8	0	42	52	30.11	SE	1	Rain.
	2	0	49	56	30.08	SE	1	Cloudy.
18	8	0	42	52	29.95	SE	1	Rain.
	2	0	43	55	29.88	E	1	Cloudy.
19	8	0	44	52	29.93	E	1	Cloudy.
	2	0	45	48	29.92	NE	1	Cloudy.
20	8	0	43	50	29.95	NE	1	Rain.
	2	0	46	49	29.95	NE	1	Cloudy.
21	8	0	43	47	29.90	E	1	Cloudy, and hazy.
	2	0	48	49	29.87	SE	1	Rain.
22	8	0	44	49	29.81	NE	1	Cloudy and hazy.
	2	0	48	50	29.86	NE	1	Cloudy.
23	8	0	45	49	29.93	N	1	Cloudy.
	2	0	46	50	29.94	NE	1	Cloudy.
24	8	0	43	50	29.93	NE	1	Cloudy.
	2	0	44	51	29.91	ENE	1	Cloudy.
25	8	0	45	50	29.72	SW	1	Cloudy.
	2	0	43	50	29.76	SE	1	Cloudy.
26	8	0	44	49	29.86	SW	1	Cloudy.
	2	0	49	50	29.74	S	1	Fair.
27	8	0	40	48	29.71	SE	1	Hazy.
	2	0	48	52	29.51	N	1.2	Rain.
28	8	0	40	49	29.47	W	1	Fair.
	2	0	42	50	29.43	SW	1.2	Rain.
29	8	0	41	48	29.49	SW	1	Fine.
	2	0	44	48	29.54	NE	1	Fine.
30	8	0	49	50	29.29	SW	3	Rain.
	2	0	51	51	29.26	W by N	3	Fine.
31	8	0	47	49	29.56	SW	2	Rain.
	2	0	49	51	29.86	SE	1	Cloudy.

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for November, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Nov.	1	8 0	39	49	30.27	W	1	Fine.
		2 0	53	52	30.37	W	1	Fine.
	2	8 0	45	49	30.23	SW	2	Cloudy.
		2 0	53	50	30.19	W	1.2	Cloudy.
	3	8 0	52	50	30.23	SW	1	Cloudy.
		2 0	50	52	30.21	SW	1	Cloudy.
	4	8 0	49	52	30.10	SW	1	Cloudy.
		2 0	52	53	30.05	W	1	Cloudy.
	5	8 0	48	53	29.97	S	1.2	Cloudy.
		2 0	52	61	29.95	S	1	Cloudy.
	6	8 0	52	61	29.82	SW	1	Fine.
		2 0	53	57	29.92	S	1	Cloudy.
	7	8 0	53	58	29.77	SW	1	Fine.
		2 0	57	63	29.63	SE	1	Cloudy.
	8	8 0	52	59	29.51	SW	1	Fine.
		2 0	52	62	29.97	SW	2	Cloudy.
	9	8 0	48	57	29.59	W	1	Fine.
		2 0	42	58	29.84	W	1	Fair.
	10	8 0	48	56	29.96	W	1	Cloudy.
		2 0	53	60	29.99	SW	1	Cloudy.
	11	8 0	50	58	29.76	SW	1	Fine.
		2 0	52	60	29.74	SW	1.2	Cloudy.
	12	8 0	49	58	29.78	SSW	1	Cloudy.
		2 0	51	61	29.65	SW	1	Cloudy.
	13	8 0	44	56	29.79	W	1	Fine.
		2 0	52	61	29.82	SW	1	Fine.
	14	8 0	54	58	29.59	SW	1	Cloudy.
		2 0	54	62	29.50	SW	1	Rain.
	15	8 0	53	59	29.33	S	2	Rain.
		2 0	51	61	26.52	W	1.2	Fine.

Rain this Month 1.110 Inches.

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for November, 1817.

1817	Time.		Therm.	Therm.	Barom.	Winds.		Weather.
	H. M.	°	without.	within.	Inches.	Points.	Str.	
Nov. 16	8 0	48	58	29.90	WSW	1		Fine.
	2 0	50	58	29.97	SW	1		Cloudy.
17	8 0	53	57	30.20	W	1		Cloudy.
	2 0	57	60	30.23	W	1		Cloudy.
18	8 0	53	60	30.25	W	1		Fine.
	2 0	56	63	30.25	W	1		Cloudy.
19	8 0	42	57	30.46	W	1		Hazy.
	2 0	45	50	30.48	E	1		Fair.
20	8 0	40	53	30.50	Wby N	1		Foggy.
	2 0	50	59	30.10	E	1		Cloudy.
21	8 0	47	57	29.95	W	1		Cloudy.
	2 0	51	59	29.88	Wby N	1.2		Cloudy.
22	8 0	44	56	30.20	NNE	1.2		Hazy.
	2 0	47	59	30.18	NW	1		Cloudy.
23	8 0	43	55	30.13	W	1		Cloudy.
	2 0	49	54	30.08	NW	1		Cloudy.
24	8 0	44	54	29.89	W	1		Hazy.
	2 0	48	58	29.81	W	1		Rain.
25	8 0	37	52	29.82	W	1		Fine.
	2 0	42	56	30.04	W	1		Cloudy.
26	8 0	45	53	30.06	W	1		Cloudy.
	2 0	49	58	30.11	W	1		Cloudy.
27	8 0	47	54	30.14	W	1.2		Cloudy.
	2 0	52	58	30.16	SE	1		Cloudy.
28	8 0	47	55	30.14	W	1		Cloudy.
	2 0	46	56	30.05	W	1		Cloudy.
29	8 0	49	56	29.99	WSW	1		Cloudy.
	2 0	52	61	29.96	W	1		Cloudy.
30	8 0	52	57	29.96	W	1		Cloudy.
	2 0	53	57	29.90	W	1		Cloudy.

Rain this Month 1.110 Inches.

METEOROLOGICAL JOURNAL

for December, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Dec. 1	8	0	54	57	29.85	WSW	1	Cloudy.
	2	0	54	60	29.75	WNW		Cloudy.
2	8	0	48	57	29.62	W	1	Cloudy.
	2	0	47	59	29.55	NW	1	Cloudy.
3	8	0	37	55	29.52	NW	1	Hazy.
	2	0	43	58	29.55	N	1	Fine.
4	8	0	36	54	29.94	N	1	Fine, rather hazy.
	2	0	38	57	29.96	N	1	Fair.
5	8	0	43	52	29.79	W	2	Cloudy.
	2	0	46	54	29.53	SW	2.3	Cloudy.
6	8	0	41	52	29.40	SSW	1	Dark and Cloudy.
	2	0	43	57	29.40	W	1	Fine.
7	8	0	37	51	29.41	W	1	Fine, rather hazy.
	2	0	42	51	29.38	W	1	Fine.
8	8	0	41	49	29.71	W	1	Cloudy.
	2	0	44	55	29.62	N	1	Rain.
9	8	0	39	51	29.13	NW	1.2	Hazy.
	2	0	39	55	29.22	NW	1	Fine.
10	8	0	34	50	29.27	WNW	1	Cloudy and hazy.
	2	0	37	54	29.37	NW	1	Fine.
11	8	0	30	47	29.57	W	1	Hazy.
	2	0	36	54	29.60	W	1	Fine.
12	8	0	27	48	29.72	W	1	Foggy.
	2	0	35	54	29.73	W	1	Hazy.
13	8	0	39	50	29.68	SW	1	Cloudy.
	2	0	42	53	29.65	SW	1	Cloudy.
14	8	0	42	50	29.53	S	1.2	Rain.
	2	0	46	50	29.50	S	1	Rain.
15	8	0	39	49	29.63	W	1	Fine.
	2	0	45	53	29.68	SW	1	Rain.
16	8	0	43	51	29.88	W	1	Rain.
	2	0	49	56	29.82	W	1	Cloudy.

Rain this Month 2.703 Inches.

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for December, 1817.

1817	Time.		Therm. without.	Therm. within.	Barom.	Winds.		Weather.
	H.	M.	°	°	Inches.	Points.	Str.	
Dec. 17	8	0	39	52	29.60	W	1	Fine.
	2	0	43	57	29.71	WSW	1	Fine.
18	8	0	44	55	28.95	WNW	1	Hazy.
	2	0	46	56	28.91	W	1	Fine.
19	8	0	42	53	28.81	W	1	Cloudy.
	2	0	46	58	28.82	W	1	Cloudy.
20	8	0	42	53	29.36	NE	1	Hazy.
	2	0	43	55	29.57	N	1	Cloudy.
21	8	0	34	40	29.54	E	1	Hazy.
	2	0	37	48	29.51	E	1	Cloudy.
22	8	0	33	46	29.54	NE	1	Cloudy.
	2	0	36	52	29.53	NE	1	Fine.
23	8	0	36	43	29.55	N	1	Cloudy.
	2	0	34	48	29.55	SW	1	Hazy.
24	8	0	32	44	29.66	NW	1	Hazy.
	2	0	34	50	29.76	NE	1	Fine.
25	8	0	32	44	29.96	NE	1	Cloudy.
	2	0	35	45	29.97	N	1	Fair.
26	8	0	30	42	30.08	NNW	1	Hazy.
	2	0	33	48	30.12	NNW	1	Fair.
27	8	0	37	44	29.85	W	1	Cloudy.
	2	0	40	48	29.96	NW	1,2	Fair.
28	8	0	35	44	29.64	NW	1	Hazy.
	2	0	37	44	29.77	N	1	Fair.
29	8	0	28	41	30.13	NWbyW	1	Hazy.
	2	0	34	47	30.16	N	1	Fine.
30	8	0	36	42	30.00	W	1	Fair.
	2	0	42	51	29.97	W	1	Rain.
31	8	0	33	45	30.05		1	Foggy.
	2	0	31	45	30.05		1	Thick fog.

1817.	Thermometer without.			Thermometer within.			Barometer.*			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	53	28	41.5	57	37	49.1	30.61	28.80	29.66	2.154
February	53	37	45.2	60	49	53.7	30.53	29.47	29.94	0.875
March	55	30	43.8	61	47	53.8	30.50	28.87	29.84	1.057
April	59	34	47.0	64	48	55.9	30.56	29.75	30.22	0.120
May	65	42	51.6	64	54	58.0	30.29	29.23	29.74	1.698
June	81	47	62.8	77	55	64.7	30.32	29.38	29.85	1.860
July	70	54	60.8	68	57	62.9	30.04	29.17	29.81	1.411
August	68	50	57.8	69	60	64.1	30.08	29.00	29.72	1.458
September	69	50	57.3	69	53	60.8	30.19	29.22	29.94	0.300
October	59	39	48.0	65	47	52.1	30.30	29.08	29.89	0.553
November	57	37	49.2	63	49	56.7	30.50	29.33	30.04	1.110
December	54	27	39.0	60	40	50.7	30.16	28.62	29.61	2.703
Whole year			50.3			56.9			29.85	15.299

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle, June 1817, 24° 17' West.

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

FOR THE YEAR MDCCCXVIII.

PART II.

LONDON,

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MDCCCXVIII.

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PHILOSOPHICAL TRANSACTIONS.

XIV. *On the parallax of certain fixed stars. By the Rev. John Brinkley, D.D.F.R.S. and Andrews professor of astronomy in the university of Dublin.*

Read March 5, 1818.

THE attention of the Royal Society has been lately called to the subject of the parallax of the fixed stars, by the astronomer royal; and as this has been occasioned principally by the results of observations which I have made at the Observatory of Trinity College, Dublin, I have taken the liberty of offering a few remarks relative to, and connected with this subject.

The Royal Society did me the honour to publish an extract of a letter to the late Dr. MASKELYNE on the parallax of α lyræ; since which time, in pursuing my observations, I have met with apparent motions in several of the fixed stars, the cause of which I was unable to explain, unless by attributing them to parallax. Among these stars α aquilæ exhibited the greatest change of place.

The results of my observations have been published in the 12th volume of the Transactions of the Royal Irish Academy

I there detailed my reasons for supposing that I could not have been misled by any source of error in the instrument, or in the mode of observing; and I trust, whatever may be the final result as to this subject, that I shall not be considered as having too hastily adopted the explanation by parallax.

I remarked, in the essay to which I have referred, my reasons for not being surprised that the Greenwich circle did not immediately confirm my results. However, after several years observations, Mr. POND was led, although he had found discordances of a similar kind, but much less than mine, to doubt the explanation by parallax, and certainly with good reason, as two instruments that might be supposed equally adapted for the examination, seemed to give different results; in consequence of which he took measures for submitting the matter to another kind of trial. He most laudably applied to the Royal Society, as visitors of the Royal Observatory; and by their assistance, and by the advantage of vicinity to the first artists, he has been enabled to put up his fixed telescopes.

Thus, unless unforeseen difficulties shall be found to exist, this question is likely to be soon decided; and certainly on many accounts, it is a most interesting question.

It is now about sixteen months since Mr. POND informed me of his doubts respecting the conclusion I had drawn from my results, and from that time I have anxiously looked to every observation that tended to confirm my conclusion, if just, or invalidate it, if wrong.

The last two years have been very unfavourable for astronomical observations; so that my opportunities always, in consequence of the cloudy atmosphere, very few, have been

during that time fewer than usual. However, I have been able to obtain some results that I shall notice farther on, which appear to coincide with my former ones as to α aquilæ, in a remarkable manner; and it is to this star that we are, I think, to look for the final decision of the question. As to α lyræ and arcturus, my results have not been so uniform as I had expected from my former observations; but as to α cygni, my recent observations are consistent with my former ones, in exhibiting the same discordance between the summer and winter observations as before, which appeared to me to point out a parallax for that star, but less than for any of the other three stars. The results of the observations of Mr. POND, by the fixed telescope, as given in the last volume of the Transactions, appear, at first sight, very decisive against the existence of any visible parallax in α cygni; but in considering these observations, a difficulty suggests itself, which if founded, will render the result deduced from α cygni and β aurigæ quite inconclusive. This, and some other points relative to the fixed telescopes, will be noticed farther on.

If it shall appear that I have been deceived by a constant source of error in my instrument as to these stars, it will be of much importance to investigate that source; and although at present I can form no conjecture as to any cause, yet, when it shall be found actually to exist, it will be incumbent on me to endeavour most strenuously to investigate the cause; and in so doing, I conceive I shall render a most acceptable service to astronomers. It will be shown, whether it be a cause that will be likely to affect other instruments. It appears at present, from the results of Mr. POND, that the

Greenwich circle is subject to a similar cause of error (supposing the discordance should not arise from parallax), and that this cause has been diminished, if not entirely done away, by reducing the internal air to the same temperature as the external.

However, from all the consideration that I have been enabled to give the subject, I am led to entertain doubts of the fitness of an instrument similar to the Greenwich mural circle, for this delicate enquiry. I do not allude to the objection stated by Mr. POND, since, as he justly observes, that is obviated by keeping the telescope fixed to the same place on the circle during a period of observations, as was the case in the observations of 1813, and as to those mentioned in the Appendix. And in respect to Mr. POND's paper, and its Appendix, as given in the first part of the Transactions for 1817, it appears to me doubtful, whether the results, if they could be exactly obtained (that is, if the elements from which they are deduced were exact), may not be such as to furnish a discordance explained by a parallax nearly equal to mine, or whether the results might not be entirely against parallax. My reasons for entertaining these doubts, will appear in the following remarks respecting the elements used in computing the index error, in instruments similar to the Greenwich mural circle.

The polar distance of a star, as observed by a mural circle, requires, besides the corrections for refraction, aberration, annual variation, &c. also the application of the index error.

This index error is determined by the mean of results deduced from observations of stars of the standard catalogue.

Let i = index error.

d = mean polar distance of a star of the standard catalogue deduced from observation.

c = mean polar distance of the same star in the catalogue.

Then

$$i = d - c$$

Let o = observed polar distance.

r = refraction.

p = parallax.

a = aberration of light.

n = nutation.

s = semiannual equation.

v = annual variation.

$$\text{Then } i = o + r + p + a + n + s + v - c$$

these quantities being applied with proper signs.

Now i partakes of the error or uncertainty of each of these quantities.

1. Let us suppose that there is no error from the observation or construction of the instrument; that is, let us suppose o exact.

2. As to refraction. Any uncertainty in the quantity of refraction affects the index error, and therefore the required polar distance of a star, although that star should be in or near the zenith. Thus the determination of the polar distance of a star in the zenith, will partake of any uncertainty in the refraction of the lower stars used for the index error.

Let us see to what this may amount as to the index error by a single star.

BRADLEY'S refractions, by which the Greenwich observa-

tions have hitherto been computed, differ from the French refractions as follows.

The French refraction at $45^{\circ}=57''5$ } therm. 50, and
 BRADLEY'S refraction at $45=56, 9$ } barom. 29,60
 and the mean diff. $= 0''6 \times \tan. \text{zen. dist. nearly.}$

But this will not affect the index error, as it equally affects r , &c.

But the effects of the change of temperature as computed by BRADLEY'S, and by the French refractions, have an important concern in this enquiry.

To deduce the actual refraction from the mean refraction for height of thermometer $= t$, the mean refraction is multiplied by $\frac{400}{350+t}$ according to BRADLEY.

According to the French tables, the multiplier is $\frac{500}{450+t}$ at least sufficiently nearly so for the 30 standard stars of Mr. POND.

The difference then between BRADLEY'S refraction and the French refraction from the change of temperature, is nearly $\frac{t-50}{2000} \times \text{mean refraction.}$

Now Procyon is one of the standard stars; and when this star passes the meridian in June, soon after mid-day, we may suppose FAHRENHEIT'S thermometer at 70° ; and when this star passes the meridian in December, near midnight, the thermometer may be at 30° . The mean refraction of this star at Greenwich, is $58''$ nearly. Therefore the refraction computed by the French table may, in summer, exceed that computed by BRADLEY'S table by $0''58$, and the contrary may

take place in winter. Hence an uncertainty in the index error, which as deduced from Procyon, might occasion a difference in the zenith distance of (ex. gr.) α lyræ in summer and winter, $= 1''.16$, bearing a considerable proportion to the supposed discordance in summer and winter. This is an extreme case: the index correction, as deduced from other stars, would not be so much affected. The polar star below the pole, is likely to be often used, and might occasion an uncertainty of about $1''$ under similar circumstances.

When I call this uncertainty, I suppose the matter is entirely doubtful as to the preference to be given to either of the formulæ.

BRADLEY'S law of change from temperature was deduced from his astronomical observations, but other astronomical observations do not contradict the law of the French formula; which has also been confirmed by physical experiments, and seems more to be depended on.

It appears then that an incorrect law may materially affect the index error, and occasion incorrect results.

It therefore seems of the first importance with respect to a mural circle, to ascertain with exactness the law of variation of refraction from change of temperature; otherwise errors will be mixed up in all the conclusions.

It will not be possible to deduce easily, by the results from BRADLEY'S refractions, the results that the French refractions would give.

For this purpose it will be necessary not only to know the mean temperature at the observations of the star, whose north polar distance is required, but also the temperature at the observations of stars by which the index error is com-

puted, which in fact is much the same as to recompute the index error,

I know not how far this may have been a source of inaccuracy in the north polar distances (p. 388, *Phil. Trans.* 1815), from the French refractions.

They seem to have been merely deduced from the column of N. P. D. by BRADLEY's refraction, and the mean heights of the barometer and thermometer, as given in page 386.

3. As to p , or the effect of parallax, we are not certain that many of the standard stars may not have a parallax in declination, amounting to a fraction of a second. This therefore so far will render the index error uncertain.

4. As to a , or the effect in declination of the aberration of light.

The maximum of aberration, pretty generally adopted of late years by astronomers, is $20''.25$. The researches of the Chevalier DELAMBRE have principally led to this. The maximum formerly used was $20''$. The former is probably more exact, but by no means certainly so. It is even possible that the maximum of aberration may be so low as $20''$, or $19''.8$. The strongest argument for $24\frac{1}{4}''$ is derived from the researches of M. DELAMBRE, respecting the reflected light from Jupiter's Satellites;* which certainly cannot be considered conclusive as to the direct light of the stars.

It seems reasonable to conclude, from an examination of Dr. BRADLEY's paper on aberration, that this matter requires farther examination, and that there is an uncertainty amounting to a quarter of a second.† If so, the index error com-

* DELAMBRE, *Astron.* Tom. 3, p. 105.

† See note (A) at the end of this paper.

puted by the pole star may be uncertain $0''.2$ or $0''.3$, in July, and the same in an opposite direction in January. Add to this, it has not been usual for astronomers to consider the variable velocity of the earth in its orbit. The effect of this in N. P. D. as to stars, the R' . of which are nearly 9° or 9° is always insensible, but not as to stars, the R' . of which are nearly 0° and 6° , and are also far from the ecliptic. This quantity is nearly

$$= \frac{ab. \text{ in N. P. D. }}{60} \cdot \cos. (\odot \text{ Long.} \sim 9^\circ 9').$$

and therefore in the pole star may amount in July to $0''.34$. Hence, from these two causes, the uncertainty in the aberration of the pole star in declination in July may be $= 0''.6$. The joint effect of these causes will be 0 in October and in January.

The index error computed by the pole star when below the pole in July, will be opposite to the above, and thus the index errors so computed at the same time may differ $1''.2$.

5. As to n , or the nutation, according to some astronomers, the nutation in declination,

$$= 7''.85 \sin. (R - \alpha) + 1''.15 \sin. (R + \alpha) \quad - \quad (1)$$

according to others,

$$8''.42 \sin. (R - \alpha) + 1''.23 \sin. (R + \alpha) \quad - \quad (2)$$

M. LAPLACE * made it (as will appear by the proper reductions) so great as

$$8''.76 \sin. (R - \alpha) + 1''.35 \sin. (R + \alpha) \quad - \quad (3)$$

(1) is the nutation according to LAMBERT's tables. The maximum of formula (3) was deduced from the mass of the moon, $= \frac{1}{58,6}$, (the earth being unity), as determined by the

* Mec. Cel. Tom. 2, p. 350.

tides at the port of Brest. LAPLACE* afterwards modified this quantity, in consequence of the determination of DELAMBRE, as to the lunar equation of the solar tables ; of the determination of Dr. MASKELYNE, as to the nutation itself from BRADLEY'S observations ; and of the determination of M. BURG, as to the parallax of the moon. These three determinations agree in deducing nearly the same mass of the moon, and induced LAPLACE to adopt $\frac{1}{68.5}$, and then the nutation will be nearly that in formula (2), which is nearly the same as that of DELAMBRE.†

LAPLACE has farther considered this subject,‡ and finds, according to a high degree of probability, that the maximum is between $9''.31$, and $9''.94$.

From the above there is evidently room for some uncertainty, which uncertainty may be doubled, by taking two stars differing 180° in right ascension.

M. DELAMBRE, although he thinks the maximum in aberration is settled, supposes the mass of the moon still subject to some uncertainty.§

6. As to s , or the semiannual equation as it is called. This = $0''.48 \sin. (2 \odot - R)$. Here, on account of the smallness of the quantity of this equation, there is no room for any material uncertainty.

There is also another equation omitted by astronomers, viz. $0''.04 \sin. (2 \gg - R)$. This cannot occasion a greater difference than $0''.08$, and therefore scarcely need be noticed even among the minute objects of this enquiry.

* *Mec. Céle. Tom. 3, p. 159.*

† *Conn. des Temps. 1818, p. 361*

‡ *Astr. Tom. 3, p. 156.*

§ *Conn. des Temps. 1810, p. 462.*

7. As to v , or the variation in declination. This consists of two parts, one the effect of the precession of the equinoxes, and the other of the proper motion of the star. The former seems determined with sufficient accuracy. Also as far as regards the stars of the standard catalogue of Mr. POND, the latter seems pretty well agreed on among astronomers. But here arises a question of some importance: is the proper motion of each star uniform? It is assumed to be so in computing it by two results separated by a long interval. A series of results sufficiently accurate, and separated by intervals sufficiently long, have not yet been obtained to ascertain this important point.

A star of the 6th magnitude, γ Leporis, seems to furnish an instance of a variable proper motion, by a comparison of the observations of BRADLEY and M. PIAZZI. There is nothing against a variable proper motion in our theories of the nature and motions of the fixed stars. Hence, another source of uncertainty in computing the index error.

8. Lastly, as to c , or the mean polar distance in the standard catalogue. This is subject to two uncertainties. The original error in the catalogue, and an uncertainty in the annual variation, as mentioned in the last article.

Notwithstanding all the care that has been used by Mr. POND in perfecting his standard catalogue, it may contain small inaccuracies, as will easily be apprehended from the observations in the preceding articles.

The uncertainties to which the index error is liable from the above causes, are independent of those to which the observation to which it is applied, is also subject. It may be

said, that these uncertainties tend to correct each other, and that the uncertainty remaining, after taking a mean of results from several stars, will be too small to be regarded. This indeed may be said as to the index error when applied to observations of the sun, moon, or planets ; but not, I think, when it is applied to investigations relative to the parallax of the fixed stars, annual variation of north polar distance, exact determination of the quantity of aberration and nutation, and these, it will be allowed, are objects of great importance in the present improved state of astronomy.

Indeed, with respect to the parallax of the fixed stars, several of these objections may be obviated by a proper selection of the standard stars. Thus the uncertainties of refraction may be avoided by using only stars near the zenith. The objection in the 8th article may be partly obviated by using the same stars for ascertaining the index error at the two periods of greatest and least parallax, and so of other uncertainties. No error as to parallax arises from neglecting the unequal motion of the earth in its orbit, as far as regards the index error computed by the same stars at the two periods. But this selection of stars will be limiting the use of the instrument, and the advantage of a mean of a number of observations lost ; and in fact, with respect to the index errors used in determining the N. P. D. of α Lyræ, α Aquilæ, and α Cygni, as given by Mr. POND, Phil. Trans, 1817, no particular selection of stars, with a view to these points, seems to have been made.

It may also perhaps be suggested, that the mural circle may be used without applying index error, as was done with

respect to the observations given in the appendix of Mr. POND's first paper. But the knowledge of the stability of the index error during six or eight months, depends on the reductions by the standard stars; and therefore the above sources of uncertainty remain. Mr. POND remarks, that between July and March the index error may have oscillated a small fraction of a second on each side the mean, and not more; so that I think no important conclusion can be deduced from the results in that appendix.

The differences between the exterior and interior temperature may have tended to exaggerate the discordance between the summer and winter observations made at Greenwich; but it may appear that sufficient observations have not been made to ascertain this point, when we consider the many other sources of uncertainty. As far as I have examined into this matter, with respect to my own observations, I cannot suppose any of my discordances materially affected by the difference of exterior and interior temperature. The room containing the circle at Greenwich is much smaller, and less lofty than the room of this Observatory, which contains both the circle and transit instrument.

I hope I have so expressed myself, that I shall be understood to mean, that I consider the results of observations hitherto made by the Greenwich circle inconclusive as to the existence or non-existence of parallax, merely from the uncertainty of the elements used in the reductions, not from any errors of the observations, or from any defects in the construction of the instrument.

I more particularly offer to the consideration of astronomers the preceding remarks, as in the present state of

astronomy, the relative fitness of instruments for ascertaining with precision the smaller motions, whether real or apparent, of the fixed stars, is an object of importance.

In instruments similar to that belonging to the Observatory of Trinity College, Dublin, the index error is found by reversing the instrument, the position of the vertical axis being ascertained by a plumb line. Thus the determination of the index error is not materially affected by any of the uncertainties above referred to. Therefore, by its principle, this instrument should appear particularly adapted for enquiries relative to the annual parallax, annual variation, &c. &c.

From the fixed telescopes we are probably to look for the final decision of the question of parallax. At first sight these seem to offer a very simple and certain criterion. However, a little consideration will point out probable sources of difficulty. Suppose the star under examination be compared with a star opposite in *R*, or with one as nearly so as can be conveniently had. Besides the uncertainty respecting the annual variation, even the uncertainty in the quantity of aberration may tend in some degree to conceal the parallax, unless the minimum of aberration in declination of each star be at the same time, and the observations are made pretty equally on both sides of this time. The star β Aurigæ has been judiciously chosen by Mr. POND to compare with α Cygni. A more proper star could not have been chosen; yet here the effect of an uncertainty in the maximum of aberration, amounting only to $\frac{1}{4}$ of a second, will have a sensible effect.

If we suppose the maximum only $20''$, as I believe the maximum used by Mr. POND is $20''\frac{1}{4}$, his winter distance for

the observations given would be increased $0''.2$, and his summer distance decreased by about the same quantity; which would make his results differ in the same direction as they should do by the effect of parallax. I do not intend by this that any argument in favour of parallax can be deduced from his results, but only to show the effect of small uncertainties.

There may be uncertainty as to the stability of the instrument during the interval which elapses between the successive observations of α Cygni and β Aurigæ, which is sometimes necessarily of several days.

This is the point before alluded to; and there appears, on examining Mr. POND's results as to α Cygni and β Aurigæ, indications of such an instability, and that to an amount that may do away the conclusion he has drawn from these observations.

The seconds of the micrometer for the same star should be the same in summer and winter, after the usual reductions, supposing no uncertainties in the elements of these reductions, supposing no parallax, and supposing no derangement in the instrument. Now, referring to Mr. POND's paper, the seconds for α Cygni are decreased by about $5''$ in summer, and those of β Aurigæ increased by nearly the same quantity. This may be concluded to arise from a derangement in the instrument by the change of temperature, as Mr. POND has mentioned no other cause. The effect of an increase of temperature, therefore, appears to be to decrease the seconds of α Cygni, and to increase those of β Aurigæ. Applying this to observations of the same day in winter, α Cygni passes the meridian near noon, and β Aurigæ near midnight, or at least

late in the evening. An increase of temperature, therefore, relative to α Cygni takes place, and the seconds in α Cygni become less than they would have been had the temperature remained the same as in the night. The sum of the seconds of α Cygni and β Aurigæ is diminished by this cause, and it would be increased by the effect of parallax. Hence this cause tends to conceal the effect of parallax in winter. In summer the passages of α Cygni and β Aurigæ are reversed as to noon, and the sum of the quantities increased by temperature and decreased by parallax.

This explanation, if justly founded, will have a tendency to diminish the value of stars nearly opposite in \mathcal{R} , which Mr. POND so judiciously selected, and by which he avoided any uncertainty from differences of parallax. As to δ Cygni, the winter observations, Mr. POND remarks, are far from satisfactory; and they seem too few and too discordant to decide any thing, even supposing we were certain of the annual variation of δ Cygni, and that it had no visible parallax.

I shall now proceed to state briefly the results of my observations up to the present time, which appear to point out parallax as to α Cygni, α Aquilæ, and α Lyræ; also the results of observations of γ Draconis.

α Cygni

The winter observations of this star cannot be materially affected by any uncertainty in the maximum of aberration, being made nearly equally on both sides of the time when parallax is greatest, and aberration = 0. But the summer observations being generally made after the time when aberration in declination = 0, the effect of a less maximum of

aberration is to increase parallax. I have therefore used for my recent observations $20''\frac{1}{4}$, and corrected my former ones, which were computed with $20''$ max. of aberration; thus using the most unfavourable quantity.

Summer Z. dist. Jan. 1, 1815.

	N°. Ob.	o . . "
1811 & 1812	23	8 45 45.71 + ,74 p
1814	10	45.97 + ,72 p
1815	20	46.12 + ,43 p
1817	14	45.22 + ,68 p
	67	

Winter Z. dist. Jan. 1, 1815.

	N°. Ob.	o . . "
1810-13	24	8 45 47.12 — ,76 p
1814-15	12	46.79 — ,80 p
1816-17	16	46.57 — ,76 p
	52	

The correct means of the preceding results being taken by attributing to each a weight proportional to the number of observations, we obtain

$$8^{\circ} 45' 46'' ,86 - ,77 p = 8^{\circ} 45' 45'' ,77 + ,63 p$$

$$\text{or } p = \frac{1,09}{1,40} = 0'',78$$

or $2 p = 1'',56$, the angle subtended by the diameter of the earth's orbit at the star.

α Aquilæ.

The stars β and γ Aquilæ pass the meridian within a few minutes of the passage of α Aquilæ; and as they are much inferior in brightness to that star, and differ less than 3 degrees in declination from it, I considered that if I could observe the three stars on the same day, the comparisons of the observations in winter and summer would furnish much information relative to the parallax of α Aquilæ.

As the stars pass so nearly together, there was not sufficient time to read off the three microscopes for each observation; I therefore, for some time, read off only the bottom microscope for γ , to be compared with the reading of the bottom microscope for α , and the three microscopes for α , giving up the observation of β . Afterwards, I only read off the bottom microscope for α , and thus was enabled to observe β . Unfortunately from the few observations to be obtained in October and November, when the sun approaches these stars, I have not succeeded hitherto in obtaining a sufficient number of observations; but my summer observations appear very satisfactory, in agreeing with the result from the former observations of these stars, which were made in the autumn of 1813, and with Mr. POND's north polar distances; whereas the summer zenith distance of α Aquilæ has been uniformly less than the winter zenith distance of that star. So that, as far as I have gone with this kind of trial, the results have been very strong in favour of the parallax of α Aquilæ. As in my recent observations of this star, only the bottom microscope has been used, I have deduced results from all my former observations of α Aquilæ from the bottom microscope only.

The conclusion as to the parallax of this star does not differ materially from my former one, where the three microscopes were used.

Summer Zenith dist. Jan. 1, 1817.

	N°. Ob.	by bottom microscope.
1808-1812	38	44° 59' 36".38 + .38 p
1814	21	36.92 + .21 p
1815-1816	22	36.10 + .30 p
1817	25	36.54 + .26 p
	106	

Winter Zenith dist. Jan. 1, 1817.

	N°. Ob.	by bottom microscope.
1808-1812	38	44° 59' 38".51 — .40 p
1813-1814	24	39.20 — .47 p
1816-1817	20	38.36 — .45 p
1817-1818	20	37.57 — .44 p
	102	

The correct means give

$$44^{\circ} 59' 36''.47 + .30 p = 44^{\circ} 59' 38''.36 - .44 p$$

$$p = \frac{1.89}{.74} = 2''.53$$

or $2 p = 5''.0$ by 208 observations.

α Lyræ.

The following are the results of my observations of α Lyræ.

My former observations are here reduced to what they would be by the French refractions, and the other observations have been reduced, taking the maximum of aberration $= 20''\frac{1}{4}$. Both circumstances tend to diminish in a small degree the parallax; but the result from all my observations gives the double parallax above $\frac{1}{2}$ a second less than I should have expected from my former observations. Whether the discordance I had found was to be attributed to parallax, or any other cause, I had expected the new results would not materially differ from my former conclusions. Although it has happened otherwise, yet an examination of the different results will, I conceive, be found not to contradict my former remarks respecting the accuracy to be attained by my instrument.

Summer Zenith dist. Jan. 1, 1811.

	N ^o . Ob.	. . . "
1808-1813	65	14 46 19.35 + .78 p
1814	20	19.87 + .78 p
1815	20	19.86 + .74 p
1816	11	20.46 + .77 p
1817	12	19.62 + .62 p
	128	

Winter Zenith dist. Jan. 1, 1811.

	Nº. Ob.	' "
1808-1813	61	14 46 20.96 — .79 p
1814-1815	20	21.00 — .78 p
1815-1816	14	19.47 — .68 p
1816-1817	15	20.88 — .76 p
1817-1818	24	20.06 — .76 p
	134	

The correct means give

$$19''.63 + .76 p = 20''.64 - .77 p$$

$$\text{or } p = 0''.66$$

or $2 p = 1''.32$, the result of 262 observations of α Lyræ.

γ Draconis.

Of this star, the mean of 53 observations in . ' "

winter gives mean Z. D. Jan. 1. 1814. = 1 52 17.55

59 observations in summer give = 1 52 17.92

This result is in a direction contrary to parallax, and therefore had I compared the differences of zenith distances of this star and α Lyræ, in summer and winter, the result would have given me a greater parallax for α Lyræ.

This conclusion is quite opposite to that of Mr. POND, and seems to me a point of much difficulty to be explained. However, from the mean of my late results as to α Lyræ, I am inclined now, to consider my former argument deduced from γ Draconis of less weight than I had attributed to it, not thinking the observations of γ Draconis sufficiently numerous.

I have thus stated the results of my observations, and the conclusions that seem to follow as to the parallax of the respective stars. The many causes that may lead, if not to actual error, at least to a high degree of uncertainty, induced me in the paper alluded to, to speak with hesitation as to my explanation. The observations of Mr. POND, as far as they go, seem to invalidate that explanation, particularly as to α Cygni and α Lyræ.

It is by observation alone that the decision can be made. No conjecture as to the relative distances of the stars can be of any material weight. The conjecture, in itself probable, that the brightest stars are nearest to us, seems opposed by another conjecture, also by itself probable, that those stars are nearest which have the greatest proper motion.

Some of the brightest fixed stars have scarcely any sensible proper motions, while those of some much smaller are very perceptible. The two stars, 61 Cygni, have each an annual proper motion of about $5''.3$ in right ascension, and of $3''$ in declination. These stars are of about the 6th magnitude, and one a little brighter than the other.

This great proper motion seemed to render it probable, that these stars are sufficiently near to us, to have a visible parallax. I accordingly made observations on one of them, but found nothing satisfactory.

Also 40 Eridani, which is of the 5th magnitude, has so great a proper motion, that we might conjecture it to be nearer to us than many of the brighter stars.

The uncertainty, therefore, respecting the relative distances, as deduced from their degrees of brightness, weakens conclusions against parallax drawn from differences of north

polar distances of stars having nearly the same right ascension, and north polar distance.

It would be an interesting circumstance, could the existence of visible parallax in any one star be ascertained, and placed beyond doubt, by the joint results of two separate instruments. The comparison of my summer and winter observations of α Aquilæ indicating so great a parallax, induces me to expect that as to this star it may yet be accomplished.

Mr. POND suggests that the effects of refraction may occasion some uncertainty as to this star. This can only arise from irregularity of refraction; and it seems scarcely possible that the mean of 100 observations can be sensibly affected thereby. My refractions have been computed from the internal thermometer placed on the instrument: had they been computed by the external thermometer, the difference between the summer and winter zenith distances of α Aquilæ would have been lessened about $0''.3$. As α Aquilæ passes the meridian near noon in winter, there is seldom much difference then between the external and internal thermometer here.

If the discordance which I have found between the summer and winter zenith distances had arisen from the different temperatures at the two seasons, it might have been expected that Aldebaran, Capella, α Orionis and Procyon would have been much more affected by this cause; as in winter they pass the meridian at night, and in summer in the day time; and therefore as to these the observations are made in the extremes of temperature.

To many, the time and labour spent in this minute enquiry, may appear wasted. Some however will justly appreciate

the exertions that have been made here and at Greenwich. Several attempts to observe the parallax of the fixed stars have failed since the time of Dr. HOOKE, and Mr. FLAMSTEAD,* and if this should end like the rest, it will be some satisfaction to have ascertained, beyond doubt, certain limits; and also, probably to have occasioned these limits to be still farther circumscribed by the observations of Mr. POND, in the event of his not confirming my conclusions.

* See note (B).

Observatory of Trinity College,
Dublin, Feb. 20, 1818.

Note (A).

Upon examination of Dr. BRADLEY's* account of the aberration, it will appear, I think, that the maximum of aberration deduced therefrom, cannot be depended on to $\frac{1}{4}$ of a second. Dr. BRADLEY afterwards mentions, in his paper on the nutation, that he had revised his computation, and states 20" as the result nearest the truth. The result from the eclipses of Jupiter's satellites, as deduced by M. DELAMBRE, is $20\frac{1}{4}$ ". The limit of the probable error of this latter determination is not easily known; but it appears to me that we ought to adopt the result of Dr. BRADLEY's revision, rather than any conclusion *we* can deduce from the data in his first paper. We have not the original observations to refer to; and it is to be remarked, that he puts down all the maxima of changes of

* Phil. Trans. xxiv, 637, or Old Abridg. vi, 149.

declination ($D-D'$) in seconds, without fractional parts, and thence deduces for each star the maximum of aberration.

	$D - D'$	$2 a$	$2 a \dagger$
γ Draconis	39"	40,4	40,3
β Draconis	39	40,2	40,2
η Ursæ Maj ^s .	36	40,4	40,4
α Cassiopeæ	34	40,8	41,1
τ Persei	25	41,0	41,4
α Persei	23	40,2	40,2
35 Camel.	19	40,2	40,2
Capella	16	40,0	39,7
	Mean	40,40	40,44

I re-computed the maximum of aberr. from column $D-D'$, and find the results as in column $2 a \dagger$, which differ a little from BRADLEY's results in column $2 a$, but differ considerably from M. ZACH's results (Conn. des Temps. 1810, p. 459) as to τ Persei and 35 Camel. However, M. ZACH seems not to have attended to Dr. BRADLEY's remark respecting τ Persei, and therefore, as to this star, his result is erroneous; and his different result from 35 Camel, must have been an error of computation.

It is evident both from the consistent results, and Dr. BRADLEY's remarks, respecting the annual variation, which he deduced from observation, that his observations from whence the above results have been deduced, must have been extremely accurate. The changes of nutation (evident by his observations, but at that time unknown to him) are included in the annual variation; and hence no source of error on this

account, except from error of observation: therefore we may conclude that had Dr. BRADLEY used the fractional parts of the seconds, the maximum would have been very accurate. We cannot now estimate the effect of this omission; we can only see that it is probable it has had a sensible effect on his conclusion; and we may suppose this to have been corrected by his subsequent revision. His words in the paper* on nutation are, "I have again examined my observations that were most proper to determine the transverse axis of the ellipse, which each star seems to describe, and have found it to be nearest to $40''$; which number I therefore make use of in the following computations." He had at first concluded it to be $20''\frac{1}{4}$. On the whole then, it seems that $20''$ is the result deduced from the direct light of the fixed stars, and $20''\frac{1}{4}$ from the solar light reflected from Jupiter's satellites. It is highly probable that future observation will find these quantities exactly equal. At present there exists an uncertainty.

Note (B.)

The results of the attempts of HOOKE and FLAMSTEAD are remarkable; the former reasoned justly on inaccurate observations, and the latter wrong on exact ones; and both imagined they had discovered a parallax. HOOKE, who erected at Chelsea a fixed telescope, 36 feet long, for observing γ Draconis, found a change of place agreeing with a considerable parallax. The great mechanical skill of Dr. HOOKE, the length of his telescope, and the precautions he took, seemed to leave no doubt.

Dr. BRADLEY, in his paper on the aberration, expresses great

* Phil. Trans. xlv, 1; or Old Abridg. x, 32.

surprise at the erroneous results of HOOKE's observations; which he, Dr. BRADLEY, had considered as exact, till they were contradicted by Mr. MOLYNEAUX's observations, and by his own. He says, "I cannot well conceive that an instrument of the length of 36 feet, constructed in the manner he describes his, could have been liable to an error of 30" (which was doubtless the case), if rectified with so much care as he represents."

It may be remarked here, that the results of the observations of α Cygni with the fixed telescope of Mr. POND, as given in the last volume of the Transactions, would in themselves appear to indicate a considerable parallax in that star, and thus produce an error similar to that of HOOKE. But Mr. POND guarded against such a source of error, by using two stars; and therefore no derangement of the instrument could affect his results, except as far as it might take place between two succeeding observations.

FLAMSTEAD's instrument, which he has described in his letter to Dr. WALLIS,* was a mural arch of 140° , by which he could observe all stars visible in his hemisphere; and observe below the pole all circumpolar stars that were not above $11^\circ\frac{1}{2}$ from the pole. He deduced the index error of his instrument by observations of the pole star corrected for refraction; not at first considering any correction for parallax as necessary: but he soon found a correction necessary, which he attributed to the effect of parallax. In this he was singularly mistaken. The result of his observations was, that the diameter of the circle described by the pole star about the pole, was $1'20''$, or $1'30''$ greater in summer than in winter.

* Wallis Oper. tom. iii. p. 701.

This we now know to be the effect of the aberration of light. Thus FLAMSTEAD's observations give the maximum of aberration $20''$, or $22''\frac{1}{2}$. The near agreement of this with BRADLEY's result is much greater than could have been expected from FLAMSTEAD's instrument; but the remarkable circumstance is, that FLAMSTEAD should have been so much mistaken in his mathematical application; and that WALLIS, who interested himself much in the question of parallax, did not point out his mistake: it can only be attributed to the great age of WALLIS, who was then in his 83rd year.

XV. *On the urinary organs and secretions of some of the amphibia.* By John Davy, M. D. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.

Read April 2, 1818.

Colombo, March 25th, 1817.

THE urinary organs of the amphibia have been imperfectly described by authors; but I am not aware that any account has hitherto been published of the urinary secretion of any of this class of animals.

Since I have been in Ceylon, both subjects have excited my attention, and on both I have had favourable opportunities of gratifying my curiosity. It may not be uninteresting to the Society, to know the results of my observations. I shall briefly state them, confined as they are at present to a few animals of four natural families.

1. *Of the urinary organs, and urine of serpents.*

The kidneys of the different kinds of serpents I have examined, resemble each other generally; though in each kind, there are minute and trifling differences. In every instance, the kidneys are very large, nearly equal in size to the liver; they are long and narrow, and very lobulated; like some of the mammalia with conglomerate kidneys, they are destitute of a pelvis; each lobule sends a small duct to the ureter, which leaves the kidney in two branches. The ureters in general terminate in a single papilla. The papilla is situated in the

cloaca between the mouths of the oviducts; it is a little elevated above the surface, and its point is directed towards a receptacle into which the urine enters. The receptacle is a continuation of the intestine, yet it may be considered distinct both from the rectum and cloaca, with both of which it communicates only by means of sphincter orifices. This conformation of parts may be seen to advantage in large species of snakes. I first observed it in the rock-snake and the rat-snake, two species of coluber, frequently found from eight to ten feet long.

The urinary ducts of serpents are frequently of an opaque white colour, from a white matter which they contain, which is visible through their transparent coats, and which may be expressed and collected from the papilla in small quantities for examination. More or less of a similar white matter is almost constantly found in the receptacle; generally it is found in soft lumps, rarely in hard masses. In the receptacle, I have always observed it pure and entirely free from fæcal matter. This solid urine, for such it is in reality, gradually accumulates in the receptacle, till it forms the masses just described. It is a long time thus collecting, from three weeks to a month or six weeks. When the bulk of the masses is so considerable as to distend the part, they are expelled by an unusual exertion of the animal, most commonly in the act of devouring its food, which it takes periodically, at intervals of from three to six weeks. The urine is voided occasionally, accompanied by, but never mixed with, fæces. When expelled, it is commonly in a soft state, of a butyraceous consistence, which it loses from exposure to the air, and becomes hard and like chalk in appearance. This change is produced, I believe, merely by the evaporation of moisture. The quantity of solid urine secreted

by snakes is very great, more even than might be expected from the size of their kidneys; it is not unusual to see masses weighing three or four ounces, voided by large snakes.

The chemical nature of this urine was such as I expected to find it; I say expected, because before I left England, I was told by Dr. PROUT, that he had examined the excrement of a serpent in London, and had ascertained that it was nearly pure uric acid; such have I found it here in every instance, in at least eight that I have tried it; and the properties of that fresh from the ureter, were precisely the same as of that contained in the receptacle, or of that voided. Before the blow-pipe, it emitted strong ammoniacal fumes, consumed without flame, and afforded only a very minute quantity of ash, consisting chiefly of phosphate of lime, and a fixed alkaline phosphate, and a little carbonate of lime; in muriatic acid it was insoluble; in warm dilute nitric acid it was soluble with effervescence; and the solution evaporated, afforded the pink residue almost peculiar to uric acid; in an alkaline ley it was soluble, and the solution was precipitated by muriatic acid. These properties sufficiently prove, that the nature of the urine of snakes is as above stated. Besides uric acid, I have not been able to detect any other ingredient, nor do I believe that the urine contains any other, with the exception of a little dilute mucus, with which it is mixed and lubricated.

2. Of the urinary organs, and urine of lizards.

I have examined the urinary organs of four different species of lizard, the gecko iguana, a large species resembling the iguana, called by the natives kobbera-guion,* and the

* For an account of this animal, see KNOX's History of Ceylon.

alligator. The shape of the kidney varies in different instances; to each ureter there is a papilla, and the papillæ are situated in the receptacle itself; and in no other respect have I been able to discover between the urinary organs of these lizards and of snakes, any material difference. Neither does the urinary secretion of these four species, and of many other species that I have examined, differ from that of snakes in its essential nature; in every instance I have found it nearly pure uric acid. The uric acid of the alligator contains a large proportion of carbonate and phosphate of lime. Two specimens of this urine, from different alligators, agreed in this circumstance; they differed however in one having no odour, and the other a strong one of musk; the former was from a very young, the other was from an older animal.

3. Of the urinary organs and urine of the turtle and tortoise.

The kidneys of the *testudo mydas*, and *geometrica*, the only species I have hitherto examined, resemble those of the preceding animals in their lobulated structure. The proportional size of the kidney of snakes is greatest; that of lizards next; and that of the animals we are now considering, least.

In the bladder, both of the turtle and tortoise, I have found flakes of pure uric acid, but in no great abundance: it was in a transparent watery fluid, containing a little mucus and common salt, but no urea or any other substance that I could detect in the small quantity on which I operated.

It is curious to observe the links by which animals, in appearance totally dissimilar, are connected together. That there should be so close an analogy between the urinary organs and secretion of the serpent, lizard, and *testudo*, is

not surprising, their organic structure, and their habits and economy being so similar; but that an analogy should exist between animals so very different in general appearance as birds and amphibia, is not a little singular, yet it is true: the urinary organs of one class, as well as the lungs, primæ viæ and genital organs, resemble those of the other, and both are peculiar in secreting uric acid; those living entirely on animal food secreting it pure.

XVI. *On a mal-conformation of the uterine system in women ; and on some physiological conclusions to be derived from it. In a letter to Sir Everard Home, Bart. V. P. R. S. from A. B. Granville, M. D. F. R. S. F. L. S. Physician in ordinary to H. R. H. the Duke of Clarence ; Member of the Royal College of Physicians, and Physician-Accoucheur to the Westminster General Dispensary.*

Read April 16, 1818.

DEAR SIR,

WHEN I proceeded, two years ago, to Paris for the purpose of studying still farther, and in a more advantageous manner, the particular branch of the medical profession I have embraced, you did me the honour to request that I would communicate to you any fact, which might occur to me during my stay in that capital, calculated to elucidate the process of generation. The good fortune I enjoyed in being admitted into the *Maternité*, or Great Lying-in Hospital, than which no other similar public institution in Europe, except perhaps that at Vienna, is better calculated to advance the study of the obstetric art ; and the frequent anatomical investigations at which I assisted, through the kindness of Mons. CHAUSSIER, one of the most distinguished professors of the Faculty of Medicine, have, I am willing to believe, placed it within my power to satisfy you on the subject of your request.

Of the many facts, however, which the numerous opportunities I possessed have brought before me, connected with the physiology of generation, none, in my opinion, deserves more

to be mentioned than the one which it is the purport of the present letter to describe; and when I reflect on the many important researches you have made on this interesting subject, I feel confident, that you will find the contents of the following pages worthy of your attention; since the case they record appears to me to stand single in the annals of descriptive anatomy. It is for this reason, therefore, that I shall confine myself to its description alone, leaving any other information I may possess on the subject, as matter for future correspondence.

Early in June 1817, I was summoned to attend the opening of the body of a woman aged 40, who died at *La Maternité*, six or seven days after delivery, of what had long been suspected to be a disease of the heart, or of some of the larger vessels. This conjecture was not ill founded; for on examining the contents of the thorax, an aneurism of the aorta was discovered, together with a considerable enlargement of the heart. As a subject for secondary consideration, it may not be improper to mention; also, that the bronchiæ were found lined by a beautifully formed membrane, entirely detached from their inner surfaces, and admitting of being extracted without deranging its tubiform structure; yet, the patient had never complained of any disorder of the respiratory organs.

Our attention, however, during dissection, was soon engaged by a still more curious anatomical arrangement, which presented itself to our view on the examination of the abdominal viscera. The womb, half concealed by the intestines, and immersed in a considerable quantity of serous fluid, being four times at least the size of what it is in the unimpregnated state, was lying in its natural position. On clearing it from all the surrounding parts, it was found that this viscus had acquired

its full developement on the right side only, where it presented the usual pear-like convexity and undulation; while the left, exhibited a *direct straight line*, scarcely half an inch distant from the centre, although more than two inches could be measured from that same point to the outline of the right side. But this is not all: the spermatic and uterine vessels, the Fallopian tube, and the ovarium, with its surrounding peritonæal folds (which on the right side were placed as usual, and in their natural state, with the regular opening into the fundus uteri) were not to be found in the left. The rudiments, for they could be called by no other name, of those appendages of the uterine system on that side, were discovered lying in the inferior part of the cavity of the pelvis, loosely connected with the cervix of the uterus; the ligamentum rotundum being inserted into the superior and interior ridge of the os pubis of the same side. In dissecting this confused mass, we perceived what may be said to have been an ovarium, shrivelled, horny, and dry, and lost within the intricacies of the substance to which it was attached.

None of the other parts of generation presented any particular appearance worthy of remark.

I have endeavoured to pourtray the peculiar arrangement of the parts above described in the accompanying sketch, taken at the time. (Pl. XVII.)

This woman had been the mother of eleven children of both sexes, and had, as I have before observed, been delivered a few days previously of twins, a male and a female.

From this latter circumstance, in particular, we are enabled, for the first time, to form a definite opinion regarding the theory which has been advanced, at different times, respecting

the cause that has been supposed to influence the procreation of the two sexes: and to answer effectually, if an answer to such a proposition can be thought necessary, the arguments adduced by some systematic writers on the subject, but more particularly by Mons. MILLOT, in a recent work, "on the art of procreating the two sexes at pleasure;" in which he lays down distinct precepts for ensuring to parents either a male, or a female succession. Most of these systems, and that of the latter physiologist more especially, are founded on the ovarium and the other uterine appendages being always double; and it is curious to reflect, that however gratuitous such an idea might seem, no proof to the contrary has ever been brought forward before, to my knowledge.

The case I have now recorded, however, expunges of course for ever from our books of physiology such an hypothesis, and leaves us to discover a more plausible reason for this peculiar and very interesting feature in the process of generation.

It moreover puts to rest our doubts respecting a phenomenon of even greater importance, the existence of which, every physiologist, who has treated on the subject, with the exception of yourself, seems to have been disposed to deny. I allude to the fact of the ova of twins, and those of different sexes too, coming from the same ovarium; when, as in the present case, the fimbriated part of the Fallopian tube must necessarily remain attached for a considerable length of time to the ovarium, so as to allow both ova to pass into the uterus, without any union taking place between their membranes at any period of their progress through the tube, and afterwards coming in contact with a different part of the womb, to which they adhere. These circumstances necessarily suppose a dis-

tance between the ova, which distance cannot exist without two distinct momenta in the propulsion of the two ova from the ovarium into the oviduct.

What obtains in cases of twins, must also take place where three, four, or even six children have been brought forth at one birth; three ova being formed in each ovarium, so near to each other, that the three points at which they were expelled, must have all come within the long continued grasp of the fimbriated bodies. Indeed it is possible, that the separation of more than one ovum at a time, from one ovarium, or from both, takes place oftener than we are led to believe, from the mere calculation of the number of complex labours. For if the ova pass into the uterus even without copulation; it is no bad logic to suppose, that some will occasionally go through that viscus, which are never impregnated at all. This is indeed proved by a greater number of corpora lutea being found in the ovaria of some females, than corresponds to the number of young they have produced; a doctrine, which I first read in your valuable communication in the Philosophical Transactions, "*On the Passage of the Ovum in Women, &c.*" when you last year gave me the paper to carry to Monsieur CUVIER, and which I was happy to find afterwards immediately adopted by that eminent naturalist, in his course of lectures on generation.

These remarks lead to another, and the last consideration on the present subject; namely, the existence of separate placentæ in cases of twins, or even triplets; which unless viewed in its true light, will make us run the risk of rendering that obscure, which in the process of generation seems perhaps the most intelligible. Instances of such occurrences are very numerous; they have been often mentioned by

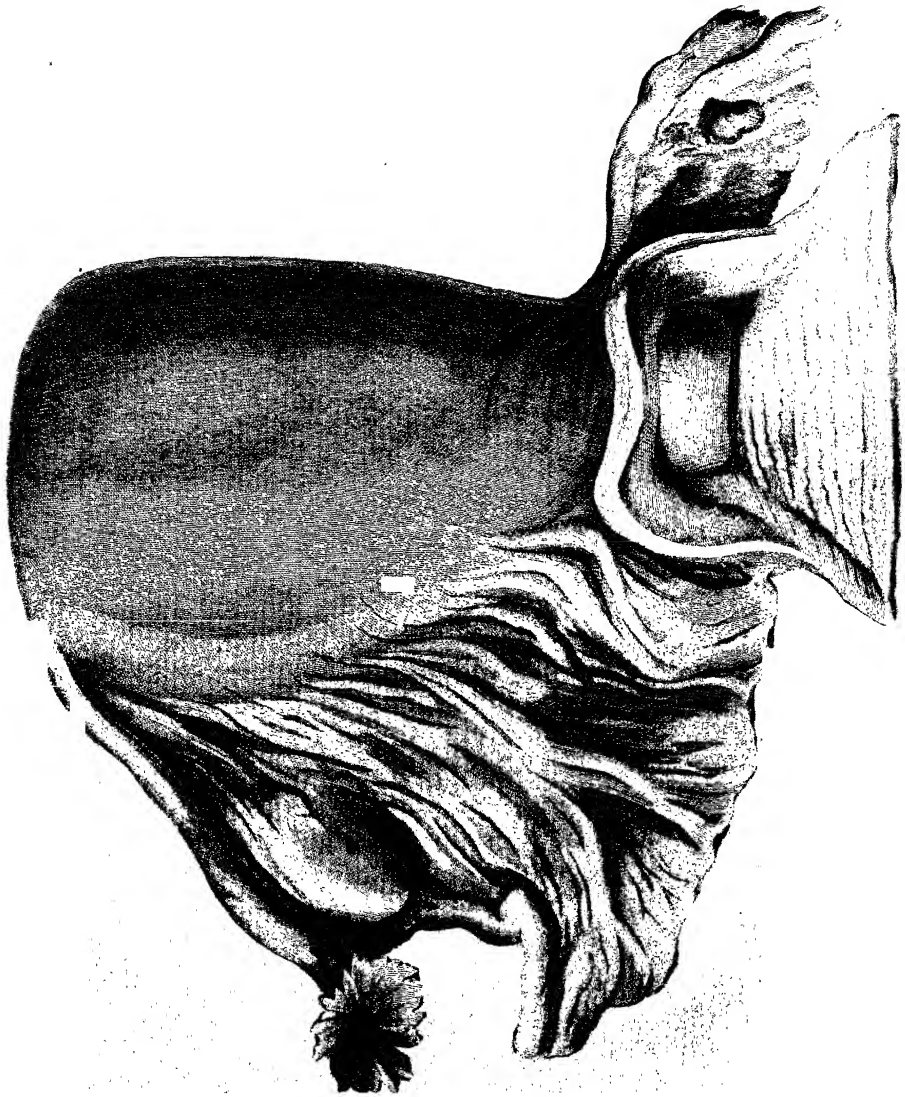
authors of veracity, and are recorded in the registers of the Great Lying-in Hospitals at Paris and Vienna. Within the last eighteen months, four cases have occurred within my own practice; and I have at this moment in my possession, the preparation of two placentæ, one of which was expelled several days before the membranes of the second were ruptured. In the case I have related, both the placentæ and membranes came away separately, and at different periods; proving still farther the soundness of the foregoing reasoning, giving a greater weight to the present observations, and explaining a number of cases in the ordinary process of generation, which have been brought forward as cases of superfetation.

In one of the volumes of the Transactions of the Royal College of Physicians, is a paper entitled "A case of superfetation," which merely goes to prove the occasional co-existence of separate ova in utero, and proves nothing farther.

The Lady, whose prolific disposition is much descanted upon in that paper, and with whom twin cases were a common occurrence, was delivered of a male child sometime in November, 1807, "*under circumstances very distressing to the parents, and on a bundle of straw;*" and again in February, 1808, (that is, scarcely three months afterwards) of another male infant, "*completely formed!*"—mark the expression, for it was not made use of in describing the first. The former died "*without any apparent cause,*" when nine days old; the other lived longer. Now, can we consider this otherwise than as a common case of twins, in

which one of the foetuses came into the world at the sixth, and the other at the ninth month of pregnancy, owing to the ova being quite distinct and separate? Had this not been the case, the *distressing circumstances*, which brought on the premature contraction of the womb, so as to expel *part* of its contents in November, as in the simplest cases of premature labour, would have caused the expulsion of the whole, or in other words, of both ova, in that same month; and we should not have heard of the second *accouchement* in the following February; which led the author of the paper in question to bring the case forward as one of superfetation, in opposition to what he has called "the scepticism of modern physiologists." Had it been proved that the child, of which the Lady in question was delivered, had *reached its full term* of uterogestation in November, and that she had brought forth another child one, two, or three months afterwards, of *equally full growth*; then a case something like superfetation would have really occurred, and scepticism would have been staggered.

I have now under my eyes a recent preparation from Mr. CHAPMAN, at Windsor, destined for Mr. CLARKE's collection; but which through the kindness of Dr. BAILLIE I have been allowed to examine for my private information; where the *complete ovum* is seen, such as it was expelled at the seventh month of pregnancy; the Lady being safely delivered of another child alive, two months afterwards. Although the first foetus was expelled at the seventh month, it was evidently of a growth of a shorter period, and had remained in the uterus dead for three months. When the case was read before the Medico Chirur-



gical Society, I ventured to give an explanation of it, similar in point to the one I have offered with regard to the Lady from Palermo, mentioned in the Transactions of the College; and I was happy to find that every one present coincided in my opinion; throwing quite out of the question every idea of superfetation.

I have the honour to be, Dear Sir,

yours truly,

A. B. GRANVILLE.

Saville Row.

XVII. *New experiments on some of the combinations of phosphorus.*

By Sir H. Davy, L.L.D. F.R.S. Vice Pres. R. I.

Read April 9, 1818.

IN a paper published in the Transactions of the Royal Society, for 1812, I have detailed a number of experiments on phosphorus, from which I deduced the composition of some of its compounds with oxygen, with hydrogen, and with chlorine. Since the appearance of this paper, various researches have been brought forward on the same subject, in which some results, differing very much from each other, and from mine, are stated. I ventured to conclude that the phosphoric acid contained double the quantity of oxygen to that in the phosphorous acid; and that phosphoric acid contained about $\frac{3}{5}$ of its weight of oxygen.

M. BERZELIUS considers the oxygen in phosphoric acid to be 128.17, and M. DULONG, 124.5, the phosphorus being 100. M. DULONG and M. BERZELIUS suppose the quantity of oxygen in phosphorous acid to be to that in phosphoric acid as 3 to 5.

The motive which immediately induced me to resume the enquiry respecting the phosphoric combinations, was M. DULONG's paper. This ingenious chemist has not only endeavoured to establish new proportions in the known compounds of phosphorus, but has likewise attempted to prove the existence of two new acids of phosphorus; and has denied several facts which I considered as sufficiently established.

The details which I have to lay before the Society in the

following pages, will serve to correct and fix, I hope, with tolerable accuracy, the proportional number or equivalent of phosphorus, and at the same time will show the truth of the general series of proportions that I assigned to its compounds. In a case where my conclusions differ so materially from those of M. BERZELIUS and DULONG, it may be supposed that I have not adopted them without considerable caution; and I have preferred my own results to theirs, only because they have been confirmed by minute and repeated experiments.

I was certain from various experiments, made both long ago and recently, and the results of which had been confirmed by Mr. BRANDE, that the proportion of oxygen, which M. DULONG assigns to phosphoric acid, is considerably smaller than that denoted by the combustion of small quantities of phosphorus in oxygen gas. I knew that minute portions of phosphuretted hydrogen were separated from phosphorus by voltaic electricity; and it occurred to me as possible, that water might be formed in the combustion of phosphorus, and separated from the phosphoric acid when it entered into saline and metallic combinations. To ascertain if this were the case, I passed phosphorus to saturation through red hot lime in a green glass tube connected with a mercurio-pneumatic apparatus: the combination took place with vivid ignition; but no elastic fluid was produced. A portion of the phosphuret of lime formed, was introduced into a tray of platinum, and heated in a glass retort filled with oxygen gas; the phosphuret of lime burnt brilliantly, and became partly converted into *phosphate of lime*; but on restoring the original temperature of the retort, there was no appearance of *vapour* or of *moisture*.

Having examined the phosphate of lime formed in this

operation, and satisfied myself that it was the same as that formed by other methods, it became evident that there were no sources of error in the experiments on the combustion of phosphorus in oxygen gas, arising from the formation or separation of water; and the only circumstance which could be urged against the accuracy of processes on this combustion, was the small quantity of materials* on which they had been made.

The vividness and rapidity of the combustion of phosphorus, renders it impossible to burn considerable quantities of phosphorus in the common way in glass vessels. Phosphuret of lime burns much more slowly and less intensely. I endeavoured to ascertain the quantity of oxygen absorbed by a given weight of phosphorus converted into phosphuret of lime; but the experiment did not succeed. Though the phosphuret of lime was in fine powder and distributed over a large surface, yet the phosphate of lime which formed and fused on the exterior, defended the interior of the phosphuret from the action of the oxygen, and prevented its combustion.

After various unsuccessful trials to convert considerable quantities of phosphorus into phosphoric acid by combinations containing oxygen, I at last thought of a very simple mode of burning phosphorus, which answered perfectly.

Phosphorus requires a considerable heat for its volatilization. By inclosing it in a small tube, so constructed that the phosphorus can burn in vapour only from the aperture of

* A source of error might be suspected in carbon combined with phosphorus; but I have been convinced by experiments made on the action of chlorine on the phosphorus I employed, that it contained no appreciable quantity of carbon. I suspect that what is often taken for carburet of phosphorus, is in reality a red oxide.

the tube, large quantities of it may be burnt by the heat of a spirit lamp in a retort filled with oxygen, and the absorption of oxygen and the quantity of phosphoric acid formed may be minutely ascertained.

The accompanying sketch (Pl. XVIII.) will give an idea of the apparatus. The neck of the little curved tube, or small distilling retort, after the phosphorus is introduced, is drawn out, and an aperture left of about $\frac{1}{10}$ of an inch; it should not be smaller, or it becomes choaked by the phosphoric acid formed. Regulating the heat by raising or lowering the spirit lamp, the combustion may be carried on slowly, or rapidly, at pleasure.

Operating in this way, I have often burnt from 5 to 10 grains of phosphorus without any accident, and ascertained exactly the quantity of oxygen absorbed: there is only one source of error—a quantity of phosphorus remains in the upper part of the tube, which cannot be burnt except by a greater heat than the retort will bear; and it is difficult to ascertain the precise weight of this, as the tube always unites with some phosphoric acid where it is red hot at its mouth; but this can be only a trifling source of error.

In these experiments, and in all the others detailed in this paper, I received much useful assistance from Mr. FARADAY of the Royal Institution; and much of their value, if they shall be found to possess any, will be owing to his accuracy and steadiness of manipulation.

EXPERIMENT I.

Six grains of phosphorus. The small tube with the phosphorus weighed before the combustion 56.5 grains; after the

combustion 50.9; so that it had increased $\frac{4}{10}$; and this increase was in great measure from phosphorus that had escaped combustion; and when this was burnt out by a strong red heat, the increase of weight of the tube was under $\frac{1}{10}$; so that at least 5.9 of phosphorus had been converted into acid: 23.5 cubical inches of oxygen were absorbed: thermometer being at 46° FAHRENHEIT; barometer 29.6 inches.

EXPERIMENT II.

Ten grains of phosphorus. The glass tube containing the phosphorus weighed 103.1 grains; after the experiment 95.6; but much phosphorus remained unconsumed. After the tube had been heated to redness, it weighed 94 grains; so that at least 8.4 grains of phosphorus were consumed in the first process. The absorption of gas was 34 cubical inches. Barometer 29.8, thermometer 47°.

EXPERIMENT III.

Ten grains of phosphorus. By weighing the tube after the experiment, and then distilling and burning the residual phosphorus, it was found that 9.1 grains of phosphorus had been burnt, which had absorbed 35.25 cubical inches of oxygen. Barometer 29.7, thermometer 49° FAHRENHEIT.

I give these experiments as the most accurate I have made. The pressure and temperature vary so little, that the corrections for them are of no importance. Supposing that 100 cubical inches of oxygen (the barometer being between 29.8 and 29.6, and the thermometer between 46° and 49° FAHRENHEIT) weigh 33.9 grains, phosphoric acid will be composed, according to the first result, of 100 phosphorus to 135 oxygen;

according to the second, of 100 to 137.2; and according to the third, of 100 to 131.3: the mean will be 100 to 134.5.

The light of the phosphorus burning in vapour in these experiments was excessively bright; yet the top of the retort never became softened; and the phosphoric acid, which increased the weight of the tube, principally combined with the glass at the aperture where it was red hot. I cannot but consider this process of burning phosphorus in the gaseous state in a great excess of oxygen, as the most accurate mode that has yet been devised for ascertaining the composition of phosphoric acid. In this instance no phosphorous acid, as I ascertained by direct trials, is formed from the vapour; and no substances are concerned except those that actually combine. M. DULONG's method of ascertaining the composition of phosphoric acid, appears to me much too complicated to afford any results approaching to accuracy. He first combines copper wire with phosphorus, by passing phosphorus over it by means of a stream of hydrogen gas; he then dissolves his phosphuret of copper in nitric acid, and determines the quantity of phosphoric acid formed by precipitation: in all of which processes sources of error may exist.

M. BERZELIUS's methods of ascertaining the composition of phosphoric acid, that of reviving gold from its oxide by means of phosphorus, and that of determining the quantities of phosphate and muriate of silver formed from perphosphorane, or the perchloride of phosphorus, appear to me still more exceptionable; yet his results on the quantity of oxygen approach nearer to mine than those of M. DULONG.

The facts which I endeavoured to establish respecting chlorine, in a paper published in the *Philosophical Transac-*

tions for 1810, show that the proportional or equivalent volume in which chlorine combines, is to that in which oxygen combines, as 2 to 1; and it follows, that 10 grains of phosphorus in forming the white sublimate, or perchloride, ought to combine with between 76 and 80 cubical inches of chlorine.

In experiments that I formerly made on this subject, by admitting chlorine to phosphorus in exhausted vessels, and ascertaining the absorption by introducing solution of chlorine, I overrated the absorption. I did not at that time know, what I have since ascertained, that a solution of chlorine in water, *apparently* saturated with chlorine, by agitation with it in long narrow vessels, will still take up more, by exposure to a great surface of chlorine in larger vessels. Under all circumstances, it is difficult to gain very precise results in experiments on the action of phosphorus on chlorine. Mercury acts so rapidly upon chlorine, that it cannot be employed in experiments in which the absorption is to be determined. When common water is used, some of the gas is absorbed by the water, and, the sublimate being a very volatile substance, its vapour always increases the volume of the residual gas. Some aqueous vapour likewise, in experiments over water, enters with the gas, which forms a volatile hydrate, the effect of which is likewise to diminish the apparent absorption of chlorine.

I have always found the absorption greatest, when I have operated in small retorts, connected by small stop-cocks with the vessel containing the chlorine, over water. Making the proper corrections for the absorption by the water, the apparent absorption has been from 35 to 38 cubical inches for every 5 grains of phosphorus.

M. DULONG's two methods of ascertaining the quantity of chlorine in the sublimate, appear to me at least as objectionable as his process for determining the composition of phosphoric acid, and liable to great errors: the first from the uncertainty of the absolute quantity of chlorine admitted; and the second, from the loss arising from the vapour of the sublimate, which must be carried off by the current of chlorine. How great a deficiency may originate from the last circumstance, is shown by the following experiment: 5 grains of phosphorus were converted into sublimate by chlorine in great excess, the remaining chlorine was displaced by passing common air through the vessel for some time, till not the slightest smell of chlorine could be perceived; the retort was then weighed, and a current of air passed through it. Though this current could hardly have replaced the air contained in the retort, yet the loss of weight was 1.7 grains, and copious vapours were produced in the atmosphere. In a second trial of the same kind, there was a greater loss of weight, and by barely exhausting the retort, and then again admitting air, there was a loss of $\frac{7}{10}$ of a grain.

When chlorine is made to act upon phosphorus over mercury not carefully dried, some muriatic acid gas is always formed; but when the mercury has been recently boiled, no effect of this kind is produced, and the vapour in the gas, forms a minute quantity of a liquid hydrate of the perchloride which by more water, is converted into muriatic and phosphoric acids, as I proved by some very delicate experiments; so that there is certainly no hydrogen denoted in phosphorus by the action of chlorine, and in their mutual action a mere binary compound of the two substances is formed.

After reflecting much upon the methods of combining chlorine and phosphorus, so as to gain correct results, it occurred to me, that in operating over water, and introducing a perfectly saturated solution of chlorine to absorb the vapour of the sublimate and of its hydrate formed from the water in the chlorine, I should gain a result nearly correct. I made an experiment in this way on 4 grains of phosphorus, in a retort containing 13 cubical inches. I ascertained the absorption, introduced into the retort a tube, containing about half a cubical inch of saturated solution of chlorine, and suffered the fluid slowly to act upon the sublimate, cooling the retort by immersion in water; I then ascertained the degree of the second absorption, which was nearly a cubical inch and a half. I likewise ascertained that water had its powers of dissolving chlorine diminished, and not increased, by uniting with phosphoric and muriatic acids, so that the apparent absorption must have been less than the real one. Adding the second absorption to the first, and making the proper corrections, the quantity of chlorine uniting to 4 grains of phosphorus was 31.9 cubical inches; barometer being 30.1 inches, and thermometer 46° FAHRENHEIT.

Rather a larger proportion would be given, if the correction for the presence of vapour had been made for some of the other experiments: and the result agrees exactly with the mean deduced from the absorption of oxygen in the formation of phosphoric acid; for, assuming that 100 cubical inches of chlorine weigh 76.5 grains, then the sublimate will consist of 1 of phosphorus to nearly 6 of chlorine; and taking the composition of phosphoric acid from this datum, it would consist of 100 of phosphorus and 135 of oxygen.

To ascertain the composition of phosphorous acid, I used a new method, that of converting the perchloride of phosphorus, or perphosphorane by phosphorus, into the chloride which affords phosphorous acid by the action of water. This is easily done by heating them together in a close retort; and it enables us to determine with certainty, which opinion is correct, *that* assuming the oxygen in phosphorous acid to be 3, or *that* which supposes it to be 2.5, the oxygen in phosphoric acid being 5.

5 grains of phosphorus were converted into perchloride in a small retort of the capacity of 6 cubical inches: it was necessary to exhaust this retort twice to remove the residual common air mixed with the chlorine, and some perchloride must have been lost during this process. A small quantity of chlorine, which could have been little more than sufficient to compensate for the loss, remained in the retort. 5 grains of phosphorus were introduced, and the retort suffered to remain filled, principally with common air; heat was very slowly applied; all the phosphorus, except an atom not so big as the head of a small pin, disappeared, and a little of the sublimate still remained, when the retort burst from the expansion of the vapour of the new chloride formed; but the chloride found on the fragments was pure, and held no phosphorus in solution.

A second experiment was made in a retort of the capacity of 11 cubical inches. 5 grains of phosphorus were converted into perchloride: the retort was twice completely exhausted, by which at least a grain and a half or two grains of perchloride must have been lost. 5 grains of phosphorus were introduced; a little of the sublimate was lost by falling into

the stop-cock of the retort; yet the conversion of the phosphorus by heat into the liquor was almost complete; there remained only a minute fragment. In this experiment, however, the liquor held phosphorus in solution. When this phosphorus was precipitated by water, and obtained with the fragment by sublimation in a small glass tube, it did not equal $\frac{7}{8}$ of a grain, and was no more than could be expected from the loss of the sublimate.

These two experiments prove distinctly that the oxygen in phosphorous acid is half that in phosphoric acid; for if the proportion had been that which M. DULONG and M. BERZELIUS indicate, 1.67 grains of phosphorus, at least, ought to have remained after the action of the sublimate.

A collateral experiment was made. 32.7 grains of the fluid chloride, made by passing phosphorus through corrosive sublimate in great excess, were acted on by water, and precipitated by nitrate of silver; the precipitate was immediately separated from the fluid, after it had been greatly diluted with distilled water. Distilled water was then repeatedly passed through it, and it was dried and fused, when it weighed 98.4 grains; which, allowing 24.5 per cent. of chlorine in horn silver, would give the composition of the fluid chloride as 24.108 of chlorine, and 8.592 of phosphorus.

The comparative quantity of precipitate in this experiment was so much less than I had found in a former experiment, that, notwithstanding the care with which the process had been conducted, I resolved to make some more experiments of the same kind. In the first, in which the decomposition by water was made in a small bottle, from which no vapour could escape, and in which I superintended the weighing and drying

of the horn silver formed, with the greatest care, 18.4 of the liquid chloride afforded only 54.5 of chloride of silver, which agrees as nearly as could be expected with the former experiment. In two other experiments, made with equal care, and in which the liquid was poured into a solution of nitrate of silver, 6 grains gave 17.1 of horn silver, and 29.4 gave 89.9 of fused horn silver.

In examining minutely the circumstances of the action of the liquid chloride, or solutions containing phosphorous and muriatic acids, or nitrate of silver, I found no difficulty in explaining the cause of the error in the former experiments. Phosphorous acid acts upon nitrate of silver, and more rapidly in proportion to its concentration, and gradually produces a copious precipitate from it; so that if there be an excess of nitrate of silver, and the precipitate be not immediately separated from the solution, there is always a considerable increase of weight. M. DULONG, and M. BERZELIUS, whose experiments agree with my former ones, *may* have been misled by a precipitation from the nitrate of silver by phosphorous acid, as I am sure *I* was. M. BERZELIUS does not state how he prepared his liquid chloride of phosphorus; but M. DULONG, who objects to my process by corrosive sublimate, and employs, instead of it, the action of chlorine on phosphorus in forming his fluid, must have been exposed to other sources of error. He speaks of acting on dry phosphorus by dry chlorine; but it must be always extremely difficult to free a gas that cannot be kept over mercury, of all its vapour; and as perchloride always forms during the action of phosphorus on chlorine, a part of which produces a fluid, and easily volatile hydrate with water, and soluble in

If proportions in the liquid chloride, this process must be very liable to error. I have never been able to form the perchloride, even from chlorine slowly passed through muriate of lime, without producing a small quantity of liquid hydrate of perchloride, which, when the solid perchloride was converted into liquid by more phosphorus, rose in vapour with it, and which, containing nearly a double quantity of chlorine, (for the water forms a very small part of it) occasions the precipitation of a much larger quantity of horn silver than the pure chloride formed from corrosive sublimate.

These various experiments on the combination of phosphorus with oxygen and chlorine, sufficiently agree with each other to afford the means of determining the proportion in which phosphorus combines with other bodies, or its equivalent number considered as an element.

If the absorption of oxygen be considered as offering the data, and phosphoric acid be supposed to consist of two proportions of oxygen, and one of phosphorus, the number representing the proportion in which phosphorus combines, will be 22.3. If phosphoric acid be considered as consisting of four proportions of oxygen, the proportional number or equivalent of phosphorus will be 44.6.

If the absorption of chlorine in forming phosphorane be made the datum, the number will be the same, 22.2, or the double 44.4. If the quantity of horn silver formed from the liquid chloride, taking the mean of all the experiments, be assumed as the datum, the number would be 23.5, or the double 47: the mean of all these proportions is 22.6, or the double 45.2; or taking away decimals, 45.

In referring to the analyses which have been made of the

different combinations of phosphoric acid, for the purpose of ascertaining if they correspond with this number, I found the data so uncertain and so discordant, that it was impossible to form any conclusions from them. The phosphate of soda, as is well known, has alkaline properties ; yet, according to M. BERZELIUS, it contains but 17.67 of soda to 20.33 of acid ; whereas it ought to contain, according to the proportion indicated by my experiments (if neutral) nearly an equal weight of soda. M. BERZELIUS mentions several combinations of baryta and lime with phosphoric acid, of which only two approach to a correspondence with the number I have given for phosphorus ; that containing 45.5 of acid to 48.7 of lime ; and that containing 39.1 of acid to 60.8 of barytes. New researches are required to explain the anomalies presented by the phosphates.

I shall give three experiments on the quantity of hydrate of potassa necessary for saturating given quantities of phosphoric acid made from given weights of phosphorus.

18 grains of phosphorus converted into phosphoric acid by combustion in oxygen, required for its saturation 47 grains of dry hydrate of potassa.

5.7 grains of phosphorus converted into acid, required 14.7 of hydrate of potassa.

5 grains of phosphorus converted into perchloride, demanded, to produce perfect neutralization, 68 grains of hydrate of potassa.

These three experiments agree so well with each other, and with the proportionate number gained from the absorption of chlorine and oxygen by phosphorus, that it is impossible not to put confidence in them.

If 13.1 be considered as the quantity of hydrate of potassa required to neutralize the phosphoric acid formed in the last experiment, and the 54.9 of hydrate remaining, be supposed to contain 43 grains of potassa, then the chlorine required to expel the oxygen from the potassa would be rather more than 40 cubical inches.

We owe to the ingenuity of M. Dulong the discovery of an acid, which he names the hypophosphorous acid, and which he supposes to contain half the quantity of oxygen in the phosphorous acid. I have satisfied myself as to the correctness of his views respecting the existence of this acid, and the properties of its compounds; but I cannot regard the method he has adopted for its analysis as entitled to confidence. He takes a given quantity of hypophosphite of soda, acts upon this by chlorine, converts the excess of chlorine into muriatic acid, precipitates by nitrate of silver and earthy salts, and from the comparison of all these data, in which some substances of uncertain composition may be concerned, draws his conclusions.

I have found that the neutral hypophosphite of barytes, when acted on by heat in close vessels, is converted into acid phosphate of barytes, disengaging an elastic fluid, which is almost entirely the hydrophosphoric gas, or phosphuretted hydrogen saturated with phosphorus. I say *almost entirely*, because in the beginning of the process, a little gas spontaneously inflammable is produced, and a minute quantity of moisture appears: and when the heat is raised to redness, a very little phosphorus is produced, probably from the decomposition of a part of the phosphoric gas. Now supposing the quantity of phosphoric acid in phosphate of baryta known,

and the quantity of phosphorus in phosphuretted hydrogen known; it is very easy, from an accurate experiment on the decomposition of the hypophosphite of baryta, to learn the composition of hypophosphorous acid.

I made two experiments on this subject; in one, 50 grains of dry hypophosphite of barytes were used, and the distillation conducted in a small glass tube. About 23.25 cubical inches of gas were produced. The loss of weight of the apparatus could not be ascertained, as unluckily a little of the phosphate was lost; a small portion of phosphorus was deposited in the upper part of the tube, from the decomposition of a minute quantity of the bi-phosphuretted gas; but this could not have equalled the $\frac{4}{10}$ of a grain, as the tube only lost $\frac{4}{10}$ by being heated to whiteness.

In the second experiment, 29 grains of the hypophosphite were used, and the loss of weight only ascertained, which was 3.5 grains. To be able to form any opinion as to the composition of the hypophosphorous acid, it was necessary to ascertain the composition of the phosphate of baryta produced in these experiments; which was easily done by precipitating a given quantity of the hypophosphite of barytes by sulphate of soda in solution. 15 grains of hypophosphite of barytes, in an experiment very carefully made, afforded 11.3 of sulphate of barytes. Now, supposing this sulphate of barytes to contain 7.4 of baryta, the hypophosphite would consist of 7.4 of baryta, and 7.6 of hypophosphorous acid; and 13.1 of the acid phosphate of baryta, formed from its decomposition, would contain 5.7 phosphoric acid, and 7.4 baryta. And in the experiment in which 29 grains of hypophosphite of baryta were decomposed, supposing the whole

loss of weight to be owing to perphosphuretted hydrogen given off, and this gas to be composed of 22.5 of phosphorus to 4 of hydrogen, or of 5.29 hydrogen to 29.76 phosphorus, and the 25.5 of acid phosphate remaining composed of 14.47 baryta nearly, and 11.03 phosphoric acid, adding the 29.76 of phosphorus to the 4.72 in the phosphoric acid, and subtracting 39, the quantity of oxygen required to form water with the 5.24 of hydrogen, the hypophosphorous acid may be conceived to be composed of 7.69 phosphorus, and 2.54, which denotes rather less than half the oxygen in phosphorous acid: i. e. as 7.43 to 1.5, an approximation nearer than could have been expected.

Assuming the composition of the phosphuretted gas to be what is stated in the preceding page, which agrees very nearly with an experiment which I formerly made, the first experiment on the quantity of gas disengaged would give a proportion of oxygen rather less than that which has been just calculated upon; but it must be remembered, that a certain quantity of common phosphuretted hydrogen is produced, which containing less hydrogen in a given volume, would sufficiently explain the difference of result.

M. DULONG has advanced an ingenious opinion, that the hypophosphorous acid *may be considered* as a triple compound of hydrogen, oxygen, and phosphorus. There is another view which may be taken of its composition, namely, that it may be a compound of phosphoric acid and perphosphuretted hydrogen. Phosphuretted hydrogen, as may be deduced from some experiments of M. DULONG, has the properties of a very weak alkali; and when expelled from the neutral hypophosphites, they become acid. This view agrees very well with the equi-

valent, or proportional numbers, which represent phosphoric acid and phosphuretted hydrogen. If it be adopted, the hypophosphites must be considered as triple compounds, analogous to the salts containing fixed alkali and earths, or ammonia and earths combined with acids.

M. DULONG imagines that the acid formed by the slow combustion of phosphorus in the air, and which I have supposed to be a mixture of phosphorus and phosphoric acids, is a peculiar acid, a chemical compound of phosphorous and phosphoric acids, which he names phosphatic acid. I cannot say that his arguments give much probability to this opinion. This substance has no crystalline form, no marked character which distinguishes it from a mere mixture of phosphorous and phosphoric acids; and as far as my experiments have gone, it is far from uniform in its composition; and phosphorous and phosphoric acids mixed together, produce a substance of exactly the same kind.

That a mixture of phosphorous and phosphoric acids should be produced by the slow combustion of phosphorus, is not surprising, when it is considered that this phenomenon is connected with different chemical processes, viz. the action of the vapour of phosphorus upon air, the action of solid phosphorus upon the elastic atmosphere, and upon the air dissolved in the moisture attracted by the acids formed; and, unless vapour be present in the air, the process of the slow conversion of phosphorus into acids soon stops.

I have mentioned in the paper to which I have referred, in the beginning of this communication, that the hydrophosphorous acid is decomposed by heat; and that phosphoric acid, and perphosphuretted hydrogen are the results. In examining the nature of the phosphoric acid formed, I find that

it contains water, so that it is a hydrated phosphoric acid. In carefully conducting the experiment, I find likewise, that a small proportion of water is given off with the perphosphuretted gas. I shall give the results of an experiment: 17.5 grains of hydrophosphorous acid were decomposed by heat in a small glass retort carefully weighed; 6.5 cubical inches of elastic fluid were generated, and the loss of the retort was 4 grains. Now, if it be assumed that the hydrate of phosphoric acid * remaining equalled 13.5 grains, and that it contained, according to the law of definite proportions, 1.88 of water, and that the bi-phosphuretted gas weighed 1.937, and consisted of 1.6446 phosphorus, and .2924 hydrogen; then the oxygen in the phosphorous acid will be to the phosphorus as 44 to 66, which is as near a result as can be expected.

For 4 proportions of phosphorous

acid are	-	-	300	or the double	150
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and 10 of water	-	-	170	or	85
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which together amount to	-	<u>470</u>	or	<u>235</u>
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which form 3 proportions of phosphoric acid	315	or	157.5
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with 3 of water to form the hydrate	-	51	or	25.5
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	<u>366</u>		<u>183.0</u>
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4 of water decomposed, of which the hydro-

gen is 8, to form with 45 of phosphorus

phosphuretted hydrogen	-	-	53	or	26.5
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3 of water given off	-	-	-	51	or	25.5
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making	-	-	-	-	470	or	235.
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* I proved it to be a hydrate by heating it with magnesia, when abundance of water was given off from it.

I have no doubt that the acid which I used formerly was drier than the acid employed in this experiment, which will account for the difference of the result. Supposing a hydrophosphorous acid could be procured, containing only the quantity of water sufficient to convert it into dry phosphoric acid, it would consist, as I have stated in my former paper on phosphorus, of four proportions of water, and four proportions of phosphorous acid.

I have adopted throughout the whole of these calculations, the supposition that the hydrogen in water is to the oxygen as 2 to 15; and consequently I have taken the number representing oxygen as 15, which is extremely convenient, as the multiples are simple, 30, 45, 60, &c. Taking the proportion of phosphoric acid in phosphate of potassa, which may be deduced from the experiments, page 329, it appears more convenient to represent the proportional number, or equivalent of phosphorus, by 45, or 45.2, than by 22.5, or 22.6, which gives facility in adopting either hypothesis of the composition of hypophosphorous acid. If it be supposed a simple compound of oxygen and phosphorus, the series of proportions in the acids of phosphorus will be

Hypophosphorous acid, Phosphorus	45	Oxygen	15
Phosphorous acid	- 45	Oxygen	30
Phosphoric acid	- - 45	Oxygen	60
or hypophosphorous acid 263	} Phosphoric acid 2 proportions	210	
		Phosphuretted hydrogen 1 prop. 53	

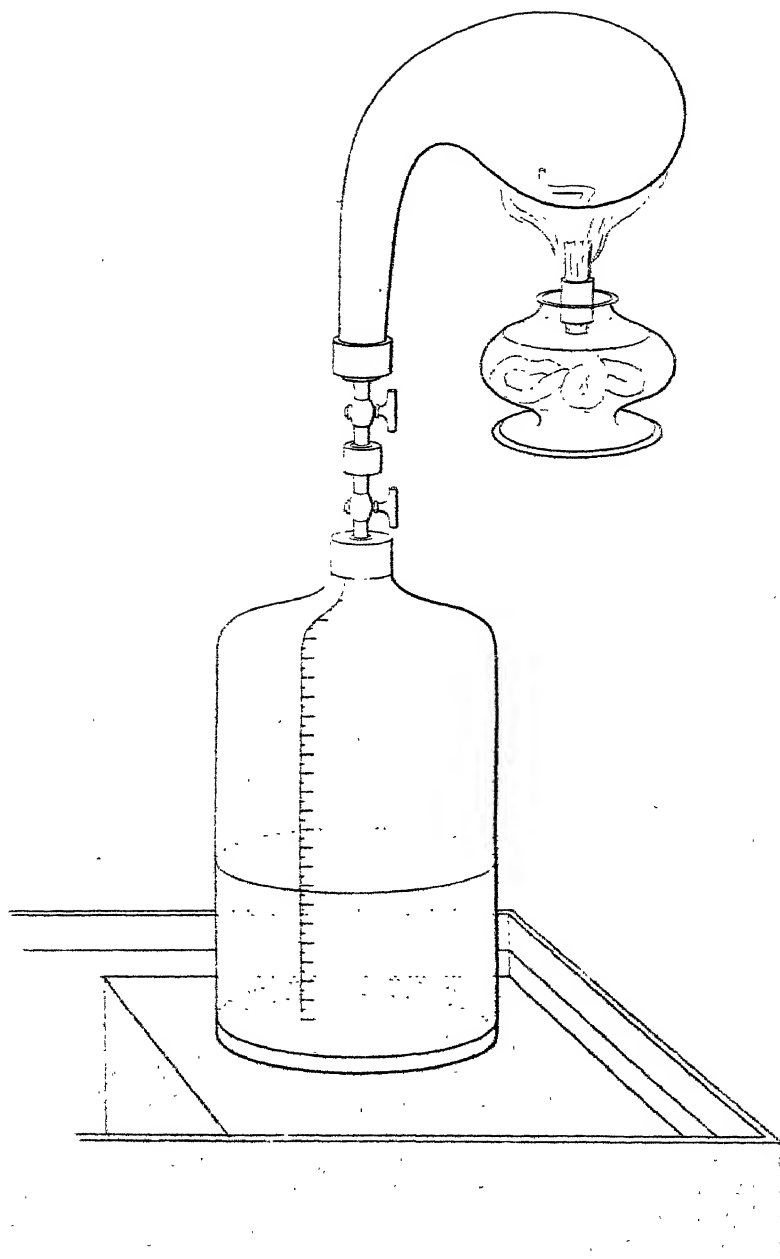
I shall conclude this paper by a few incidental observations on the compounds of phosphorus.

M. Dulong states that no phosphorous acid is formed when phosphorus is burnt in excess of oxygen or atmospheric air;

as, he says, I have asserted. I cannot find that I have any where made such an assertion; but notwithstanding what M. DULONG pretends, the assertion is true, as the following experiment will prove. Half a grain of phosphorus was set fire to in a retort containing 16 cubical inches of common air; the acid products were washed with distilled water, and passed through a filter, and evaporated. When the acid became nearly dry, small globules of phosphuretted hydrogen were disengaged from it, indicating the presence of phosphorous acid. The experiment was repeated two or three times, care being taken to separate the red powder which has been considered as an oxide of phosphorus, and always with the same result.

Whenever phosphorus is inflamed, and suffered to become extinguished in oxygen gas in excess, unless the *product* is strongly heated after the spontaneous combustion is over, *an acid*, of which the hydrate produces phosphuretted hydrogen by heat, is always found in the products; and this acid is probably produced by the action of the solid phosphorus on the phosphoric acid in contact with it. This fact, and the circumstance, that much phosphorus acid is produced by the combustion of phosphorus in rare air, renders it almost certain that the phosphorous acid is a direct combination of phosphorus and oxygen, and destroys an idea which might otherwise be formed from the phenomena of the decomposition of its hydrate, namely, that it is a compound of three proportions of phosphoric acid, and one of phosphuretted

M. Dulong and M. BERZELIUS speak of freeing phosphorane, or the liquid chloride of phosphorus, from phosphorus,



by distillation. In experiments made in the laboratory of the Royal Institution, in which it has been twice carefully distilled at a low heat, it has still contained minute quantities of phosphorus.

It has been supposed that dry phosphoric acid is fixed at a white heat; but I find that this is not the case: it rapidly rises in vapour at this temperature, and evaporates even at the point of fusion of flint glass: and the hydrate of phosphoric acid is susceptible of being volatilized at a much lower temperature.

In converting the solid sublimate composed of phosphorus and chlorine into the liquid compound, when the phosphorus is first fused in contact with the sublimate, a yellow crystalline mass is formed, which, when acted on by a higher degree of heat, affords the liquid chloride, which rises from it in vapour, and leaves phosphorus behind. It is possible that this yellow solid is a compound of phosphorus and chlorine, containing half as much chlorine as the liquid. Should this be proved to be the case by future experiments, it will give weight to the idea, that the hypophosphorous acid is a binary compound of oxygen and phosphorus.

XVIII. *New experimental researches on some of the leading doctrines of caloric; particularly on the relation between the elasticity, temperature, and latent heat of different vapours; and on thermometric admeasurement and capacity.* By Andrew Ure, M. D. Communicated by W. H. Wollaston, M. D. F. R. S.

Read April 30, 1818.

Glasgow, July 1817.

1. *On the elastic force of vapours, with new formulæ to determine it at any temperature; and a review of those given by DALTON and BIOT.*

THE phenomena attending the conversion of liquids into elastic fluids, were first accurately investigated by Dr. BLACK. He observed in the rising of vapour, and melting of ice, a beautiful system of relations, connecting and modifying the grandest operations of nature, while they were destined to afford new principles for the advancement of the arts. If it be the prerogative and characteristic of genius, to discover in the most familiar, or, as some would say, vulgar phenomena, that mystic chain of causation, which had eluded all other eyes, unquestionably, the doctrines of latent heat entitle their author to rank in the first class of philosophers.

Dr. BLACK directed his attention principally to the establishment of the general laws, which he placed on an immoveable basis; leaving to his pupils, the subordinate task of investigating their individual applications. Hence, the elastic forces of the vapours, arising from different bodies, at different temperatures, seem to have occupied him very little, if at all.

This subject was examined, however, with great ability, by two of his most distinguished friends, Professor ROBISON and Mr. WATT. The investigations of the former were published in the *Encyclopædia Britannica*, article *steam*; while we have still to regret our ignorance of those executed by the latter philosopher, with probably a more complete apparatus, and more extensive views. We are indebted to him, indeed, for some curious observation on the latent heat of steam, at different temperatures, which make us lament more, the want of those on the elastic forces themselves.

Mr. DALTON, whose peculiar speculations on caloric and météorology led him to study the formation and variable elasticity of vapour with great attention, has since then favoured the world with many excellent dissertations, and is now reckoned the first authority on the subject. Mr. DALTON's experiments on the steam of water were carried no higher than its ordinary boiling point; but from the observed progression of its elastic force he investigated a formula, and calculated from it a table for the higher temperatures.*

In the second number of the *Journal Polytechnique*, M. BETANCOURT, an eminent Spanish engineer, long resident at Paris, published a set of experiments on the same subject, the results of which differ from those of Mr. DALTON in many particulars, but most remarkably in the higher part of the scale.

Having had my mind often called to this important inquiry in the course of my public lectures on the applications of Science to the Arts, an apparatus of a very simple nature occurred to me, about two years ago, by which I hoped to be

* Manchester Memoirs, vol. v. p. 563.

able to determine, with great precision, the elastic forces of vapours at any temperature, from zero of FAHRENHEIT, to a much higher degree of heat than even BETANCOURT seems to have reached. The experiments were made soon after that time, but circumstances have till now prevented me from arranging them for publication.

With BETANCOURT's apparatus I am not acquainted, having seen only the brief table of results, inserted in our systematical works on chemistry. Professor ROBISON's consisted of a strong boiler or digester, containing the water, and furnished with three small apertures; the first receiving the bulb of a thermometer, the second covered with a safety valve, and the third having a barometer tube attached. At first I used a similar construction, but finding it hazardous, and somewhat unmanageable in the high heats, and difficult to render air tight in the lower temperatures, I abandoned it, after some unsatisfactory trials. At the low degrees of heat, the vacant part of the barometer tube introduces errors, since it has not the temperature of the boiler; and the bulb of the barometer, used in high heats, occasions a similar fallacy in the determination of the true elasticities.

Still, however, it was ingeniously conceived, and the results furnish good approximations, creditable to the celebrated experimenter.* They agree nearly with those of BETANCOURT, being obtained probably in a similar way. The method adopted by Mr. DALTON, is recommended by an elegant simplicity. It is merely a common barometer, into which a little of the vapour-giving liquid is introduced, so as to moisten, and float above the mercury. The vapour which is

* See *Encyclopædia Britannica*, vol. xvii. p. 739, 2d Edition.

generated, depresses more or less the barometric column. Hence, by subjecting the liquid to successive degrees of temperature, the corresponding depressions of the barometer, or elasticities of the vapour, are obtained.

The only difficulty in this mode of operating, is to bring a considerable length of vertical tube to an uniform temperature.

Mr. DALTON, well aware of this source of error, obviated it in a great measure, by taking a series of different tubes, decreasing in their lengths with the increasing expansions of the vapour, and concomitant descent of the mercurial column. In several experiments conducted on this plan, I found it scarcely possible to obtain results rigidly corresponding with each other, when the column of vapour, exposed in the barometer tube to the influence of surrounding heat, exceeded two inches in length.

M. BIOT, in his system of physics recently published, while he adopts Mr. DALTON's results as the basis of his reasoning, treats fully of this difficulty, and suggests an ingenious means of avoiding it. "We have had occasion several times to remark," says he, "that the temperature of a mass of liquid which cools in the air, is not entirely the same at the bottom, as it is at the top of the vessel; because the colder particles subside into the lower strata, by the excess of their weight. Thus the temperature of the column of hot water, which surrounds the tube in the preceding experiment, cannot be rigorously uniform throughout its whole height. We may endeavour to render it equal, by agitating and mingling the different strata of which it is composed; but this would

“ be attended with no little difficulty. It would be better to
 “ have several thermometers suspended at different heights,
 “ in the body of the water, and to take the arithmetical mean
 “ of their indications. Or otherwise, which would probably
 “ be more exact, we might employ a thermometer having a
 “ cylindrical bulb, equal in length to the column of vapour.
 “ It would then be necessary that the column of water should
 “ rise sufficiently above this vapour to allow the thermo-
 “ meter bulb to be equally immersed, or we must make on
 “ its indications the small correction mentioned p. 59, in
 “ order to reduce the temperature of the cylinder of mercury,
 “ to the temperature of the reservoir. The employment of
 “ such a thermometer may appear at first sight sufficiently
 “ difficult, since it seems that the length of the cylindrical
 “ reservoir must be very considerable, if the elastic force of
 “ the vapour be great.*”

He then proceeds to show how this difficulty may be ob-
 viated (as indeed it had previously been by Mr. DALTON), by
 taking barometer tubes successively shortened, as the force
 of the steam is augmented by heat. He proposes to use four,
 between the freezing and boiling points of water, each being
 two decimeters, or nearly 8 inches long, and the thermometer
 bulb having also that length. The plan which I imagined, as
 it completely obviates the source of errors arising from the
 large and variable space occupied by the vapour, supersedes
 the necessity of employing M. BIOT's singular remedy. It
 likewise avoids other complications, introduced by the heat-
 ing and consequent elongation of the mercurial column itself

* *Traité de Physique. Tome I. p. 268.*

attending all the other methods; and scarcely capable of being exactly appreciated at high temperatures with the apparatus of Professor ROBISON.

The space over which the vapour extends in my instrument, need never be greater than half an inch of a barometer tube, against the side of which part the oblong bulb of a delicate thermometer rests, so as to indicate the true temperature. And though the liquid and incumbent vapour are thus always restricted to the summit of the barometer tube, we can, notwithstanding, measure its progressive range of elasticity, from zero of FAHRENHEIT to one hundred, or even two hundred, degrees above the boiling point of water, from an elasticity of 0.07 of an inch, to that capable of sustaining 14 feet, or even 36 of mercury. Fig. 1 (Pl. XIX.) represents the construction employed for temperatures under and a little above the boiling point. Fig. 2 and 3 are used for higher temperatures; the last is the more convenient of the two. Each was suspended from a lofty window ceiling, and placed in a truly vertical position by means of a plumb line.

One simple principle pervades the whole train of experiments; which is, that the progressive increase of elastic force developed by heat from the liquid, incumbent on the mercury at $l' l''$, is measured by the length of column which must be added over L , the primitive level below, in order to restore the quicksilver to its primitive level above, at l . These two stations, or points of departure, are nicely defined by a ring of fine platina wire twisted firmly around the tube.

At the commencement of the experiment, after the liquid well freed from air has been let up, the quicksilver is made a tangent to the edge of the upper ring, by cautiously pouring

mercury in a slender stream into the open leg of the syphon D. The level ring below is then carefully adjusted.

From the mode of conducting my experiments, there remained always a quantity of liquid in contact with the vapour, a circumstance essential to accuracy in this research.

Suppose the temperature of the water or the oil in A to be 32° F., as denoted by a delicate thermometer, or by the liquefaction of ice; communicate heat to the cylinder A, by means of two argand flames, playing gently on its shoulder at each side. When the thermometer indicates 42° , modify the flames or remove them, so as to maintain an uniform temperature for a few minutes. A film or line of light will now be perceived between the mercury and the ring at *l*, as is seen under the vernier of a mountain barometer when it is raised a few feet off the ground. Were the tube at *l* and L of equal area, or were the relation of the areas experimentally determined, then the rise of the quicksilver above L would be one half, or a known submultiple of the total depression, equivalent to the additional elasticity of the vapour at 42° above that at 32° . Since the depressions, however, for 30 or 40 degrees in this part of the scale are exceedingly small, one half of the quantity can scarcely be ascertained with suitable precision, even after taking the above precautions. And besides, the other sources of error, or at least embarrassment, from the inequalities of the tube, and from the lengthening space occupied by the vapour, as the temperature ascends, render this method of reduction very ineligible.

By the other plan we avoid all these evils. For whatever additional elasticity we communicate to the vapour above *l*, it will be faithfully represented and measured, by the

mercurial column which we must add over L, in order to overcome it, and restore the quicksilver under *l* to its zero or initial level, when the platina ring becomes once more a tangent to the mercury.*

At E a piece of cork is fixed, between the parallel legs of the syphon, to sustain it, and to serve as a point by which the whole is steadily suspended.

For temperatures above the boiling point, the part of the syphon under E is evidently superfluous, merely containing in its two legs a useless weight of equipoised mercury. Accordingly for high heats, the apparatus fig. 2, or 3, is employed, and the same method of procedure is adopted. The aperture at O, fig. 3, admits the bulb of the thermometer, which rests as usual on *l''*. The recurved part of the tube is filled with mercury, and then a little liquid is passed through it to the sealed end. Heat is now applied by an argand flame to the bottom of C, which is filled with oil or water, and the temperature is kept steadily at 212° for some minutes. Then a few drops of quicksilver may require to be added to *D''* till *L''* and *l''* be in the same horizontal plane. The farther conduct of the experiment differs in no respect from what has been already described. The liquid in C is progressively heated, and at each stage mercury is progressively added over *L''* to restore the initial level, or volume at *l''*, by equipoising the progressive elasticity. The column above *L''* being measured, represents the succession of elastic forces. When this column is wished to extend very high, the vertical tube requires to be placed for support in the groove of a long wooden prism.

* Rings of other metals will not suit; for their expansions being much greater than that of glass, they become loose with the elevation of temperature.

The height of the column in some of my experiments being nearly 12 feet, it became necessary to employ a ladder to reach its top. I found it to be convenient in this case, after observing that the column of vapour had attained its primitive magnitude, to note down the temperature with the altitude of the column ; then immediately to pour in a measured quantity of mercury nearly equal to three vertical inches, and to wait till the slow progress of the heating again brought the vapour in equilibrio with this new pressure, which at first had pushed the mercury within the platina ring at *l'''*. When the lower surface of the mercury was again a tangent to this ring, the temperature and altitude were both instantly observed.

This mode of conducting the process will account for the experimental temperatures being very often odd and fractional numbers. I present them to the public as they were recorded on the instant in that particular repetition of the experiment which I consider most entitled to confidence. To trim and fashion the results into an orderly looking series, would have been an easy task ; but in my opinion this is a species of deception very injurious to the cause of science, and a deviation from the rigid truth of observation, which ought never to be made for any hypothesis. We shall afterwards have ample opportunities of exposing the fallacy of such premature geometrical refinements.

The thermometers were constructed by CREIGHTON, with his well known nicety, and the divisions were read off with a lens, so that $\frac{1}{10}$ of a degree could be distinguished. After bestowing the utmost pains in repeating the experiments during a period of nearly two months, I found that the only way of removing the little discrepancies, which crept in

between contiguous measures, was to adopt the astronomical plan of multiplying observations and deducing truth from the mean. It is essential to heat with extreme slowness and circumspection, the vessels, A, B, C. One repetition of the experiment occupies on an average 7 hours.

TABLE I.

The elastic force of the vapour of water in inches of mercury.

Temp.	Force.	Temp.	Force.	Temp.	Force.	Temp.	Force.	Temp.	Force.	Temp.	Force.
24°	0.170	115°	2.820	195°	21.100	242°	53.600	270	86.300	295.6	130.400
32	0.200	120	3.300	200	23.600	245	56.340	271.2	88.000	295	129.000
40	0.250	125	3.830	205	25.900	245.8	57.100	273.7	91.200	297.1	133.900
50	0.360	130	4.366	210	28.880	248.5	60.400	275	93.480	298.8	137.400
55	0.416	135	5.070	212	30.000	250	61.900	275.7	94.600	300	139.700
60	0.516	140	5.770	216.6	33.400	251.6	63.500	277.9	97.800	300.6	140.900
65	0.630	145	6.600	220	35.540	254.5	66.700	279.5	101.600	302	144.300
70	0.726	150	7.530	221.6	36.700	255	67.250	280	101.900	303.8	147.700
75	0.860	155	8.500	225	39.110	257.5	69.800	281.8	104.400	305	150.560
80	1.010	160	9.600	226.3	40.100	260	72.300	283.8	107.700	306.8	154.400
85	1.170	165	10.800	230	43.100	260.4	72.800	285.2	112.200	308	157.700
90	1.360	170	12.050	230.5	43.500	262.8	75.900	287.2	114.800	310	161.300
95	1.640	175	13.550	234.5	46.800	264.9	77.900	289	118.200	311.4	164.800
100	1.860	180	15.160	235	47.220	265	78.040	290	120.150	312	167.000
105	2.100	185	16.900	238.5	50.300	267	81.900	292.3	123.100	Another expert.	
110	2.456	190	19.000	240	51.700	269	84.900	294	126.700	312°	165.5

The apparatus employed in obtaining these results, has the peculiar advantage over all others, that the mercurial column is never heated. It is the concurrent opinion of all chemical philosophers, that caloric travels downwards in liquids with extreme slowness and difficulty. Indeed, Count RUMFORD's experiments led him to infer that heat could not descend in fluids at all.

It is evident that in my constructions, figures 1, 2, and 3, only that small portion of quicksilver, within the vessels

A, B, and C, will be affected by the heat, but the measuring column is beyond the reach of its influence.

A surprising accordance will be perceived between my numbers, and those given by Mr. DALTON between 32° and 212° , though mine were obtained with a different modification of apparatus. Above the boiling point, where the table of Mr. DALTON is deduced from calculation, the accordance soon ceases. But as my apparatus and mode of using it were precisely the same as in the former part of the range, my results, if entitled to confidence in the one case, must be so in the other. At 280° BETANCOURT's number and mine are not much different, the former being 105 inches, the latter 102. Being perfectly convinced, by repeating the experiments in different circumstances, that Mr. DALTON's ratio of progression, though apparently accommodated to the intervals between 32° and 212° , could not serve for the higher ranges,* I endeavoured to discover a simple rule of more general application. It is above 212° , indeed, that for the purposes of art, the knowledge of the force of steam is required.

I first tried the differential method, so useful for determining the distant links of a concatenated series.

Without doing much violence to the above numbers, the forces corresponding to 100° , 110° , 120° , 130° , 140° , and 150° , may be written in a series of which the 5th order of differences = 0. Then if d' d'' d''' d^{iv} d^v , represent the first terms, in the first, second, third, fourth, and fifth order of differences, the n^{th} term of the series is

* Dr. YOUNG remarks on DALTON's ratio, "It is certain that this cannot be the law of nature, since about 394° the elasticity would become uniform, and then decrease, if the law be true." YOUNG's Natural Philosophy, 4to. vol. ii. p. 398.

$$a + \frac{n-1}{n-1} \cdot d', + \frac{n-2}{n-1} \cdot \frac{n-2}{2} \cdot d'', + \frac{n-2}{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot d''', +$$

$$\frac{n-2}{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot \frac{n-4}{4} \cdot d''', + \&c.$$

In the above series for steam, $d' = 0.65$, $d'' = 0.19$, $d''' = 0.04$, $d^{iv} = 0.01$, $d^v = 0$. $a = 1.92$.

Example 1st. To determine the 8th term in the series, or the elastic force at 8×10 , above 90° , (the first term 100° being included) or at 170° .

Here $n = 8$

$$\begin{array}{rcl} a + \frac{n-1}{n-1} \cdot d' & = & 1.92 + 4.55 = 6.47 \\ \frac{n-2}{n-1} \cdot d'' & = & 4.08 \\ \frac{n-2}{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot d''' & = & 1.40 \\ \frac{n-2}{n-1} \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot \frac{n-4}{4} \cdot d^{iv} & = & 0.35 \\ & & \hline & & 12.30 \end{array}$$

Observation gives 12.05, forming a good accordance.

Example 2. Required the 10th term, or $n = 10$. For 190° F.

$$\begin{array}{rcl} a + \frac{n-1}{n-1} \cdot d' & = & 7.77 \\ \frac{n-2}{n-1} \cdot d'' & = & 6.84 \\ n-1 \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot d''' & = & 3.36 \\ n-1 \cdot \frac{n-2}{2} \cdot \frac{n-3}{3} \cdot \frac{n-4}{4} \cdot d^{iv} & = & 1.26 \\ & & \hline & & 19.23 \end{array}$$

At 190° experiment makes it 19.00, still coinciding nearly.

By the same equation we find the 20th term or for 290° to be 124.28, while experiment gives 120.15, showing a difference of 4.13 inches. At a higher point the error becomes greater. We here see that a geometrical series may coincide

apparently through a considerable range with experiment, and yet be inaccurate when farther extended.

Dissatisfied, therefore, with this approximation, I prosecuted the inquiry, and had the happiness to discover a very simple and beautiful ratio, which will actually apply through an extensive scale of temperature, and is incomparably easier in practice than the preceding rule. The elastic force at $212^{\circ} = 30$ inches being divided by 1.23, will give the force for 10° below; this quotient divided by 1.24, will give that for 10° lower; and so on progressively. To obtain the forces above 212° , we have merely to multiply 30 by the ratio, 1.23 for the force at 222° ; this product by 1.22 for that at 232° , and thus for each successive interval of 10° above the boiling point.

Thus $30 \times 1.23 = F_{222^{\circ}}$. $30 \times 1.23 \times 1.22 = F_{232^{\circ}}$,
using F to denote the force at any temperature n , according to the notation of LAPLACE.

By departing from the point of 210° F., we shall obtain results equally accurate, but more convenient for comparison with the experimental table. The following numbers exhibit the correspondence of this ratio with actual observation.

TABLE II.

Observed elasticity of aqueous vapour compared with the ratios.

Temp.	Calcul. Force.	Expert.	Temp.	Calcul. Force.	Expert.	DALTON.	BETANC.	ROBISON.
210°	28.9	28.9	210°	28.9	28.9	28.84	28.8	28.65
200	23.5	23.6	220	35.54	35.54	34.99		35.80
190	19.0	19.0	230	43.36	43.10	41.75	45.5	44.70
180	15.2	15.16	240	52.46	51.70	49.67		54.90
170	12.07	12.05	250	62.95	61.90	58.21		66.80
160	9.50	9.60	260	74.91	72.30	67.73	80.17	80.30
150	7.42	7.58	270	88.39	86.30	77.85		94.10
140	5.75	5.77	280	103.41	101.90	88.75	105.12	105.90
130	4.42	4.36	290	119.95	120.15	100.12		
120	3.37	3.33	300	137.94	139.70	111.81		
110	2.55	2.45	310	157.25	161.30	123.53		
100	1.92	1.86	320	177.70		135.00		
90	1.43	1.36						
80	1.06	1.01						
70	0.77	0.726						
60	0.56	0.516						
50	0.40	0.36						
40	0.28	0.25						
30	0.20	0.19						
20	0.14	0.14						
10	0.098							
0	0.068							
						Temp.	BETANC.	ROBISON.
						32°	0.0	0.0
						50		0.12
						80	0.81	0.82
						100	1.65	1.60
						120	2.95	3.00
						140	5.00	5.15
						160	9.00	8.65
						180	14.00	14.05
						200	22.50	22.62

The rule on which the preceding table is formed, may be expressed in a manner better fitted to give *directly* the elastic force corresponding to any given temperature moderately distant from 212°. It becomes also more accurate.

Let r = the mean ratio between 210° and the given temperature; n = the number of terms (each of 10°) distant from 210°; F = the elastic force of steam in inches of mercury.

Then, $\text{Log. of } F = \text{Log. } 28.9 \pm n. \text{ Log. } r$; the positive sign being used above, the negative below 210°.

Or by common arithmetic, multiply or divide 28.9, according as the temperature is above or below 210°, by the mean ratio, involved to a power denoted by the number of terms. The product or quotient is the tension required.

Example 1st. The temperature is 140°. What is the corresponding elasticity of the vapour from water heated to that point?

140° is 7 terms of 10° each under 210°; 1.26 is the mean ratio = $\frac{1.23 + 1.29}{2}$; and, consequently, $r = 1.26$; $n = 7$.

$$\text{Log. } 28.9 = 1.46090$$

$$\text{Log. } 1.26 \times 7 = 0.10037 \times 7 = \underline{0.70259}$$

0.75831, which is

the logarithm of - - - 5.732 inches.

Experiment gives - - - 5.77, difference .04, inconsiderable.

Example 2. What is the tension of steam at the temperature of 290°?

$$r = \frac{1.23 + 1.16}{2} = 1.195 \quad n = 8$$

$$\text{Log. } 28.9 = 1.46090$$

$$8. \text{ Log. } r = 8 \times 0.07737 = \underline{+ 0.61896}$$

$$\text{Log. of } 120.02 \text{ inches } 2.07986$$

$$\text{At } 290^\circ \text{ by experiment } = 120.15$$

Example 3. Temperature 250°. Force of steam in contact with water?

$$r = \frac{1.23 + 1.20}{2} = 1.215 \quad n = 4$$

$$\text{Log. } 28.9 = 1.46090$$

$$4 \text{ Log. } r = 4 \times 0.08458 = \underline{+ 0.33832}$$

$$\text{Log. of } 62.98 = 1.79922$$

$$\text{At } 250^\circ \text{ Experiment } 61.90$$

At these high heats, it is very possible that the experiment may be in error by 1 inch, which is the whole difference here. About half a degree of FAHRENHEIT misnoted, would give this deviation.

Such a correspondence, therefore, of observation with the calculated results, shows that we have found a rule of perfect accuracy for all purposes of engineering, &c. If I am asked whether this formula coincides at every link with the chain of nature, I freely acknowledge, that I do not imagine it strictly so to do. But still it affords approximations such, that within moderate limits, I cannot tell whether to place more confidence in them, or in those found by experiment. It has moreover the rare advantage of being extremely simple, and level to the capacity of all practical men.

In Bior's excellent work above quoted, where many of the hitherto vague disquisitions of physical science have been happily brought within the pale of geometry, this celebrated philosopher has deduced, from Mr. DALTON's experiments on the force of steam, a general formula for determining its elasticity at any temperature.

In investigating this formula, he represents the decrease of the logarithms of the elastic forces by a series of terms of the form $an + bn^2 + cn^3$; $a b c$ being constant coefficients.

$$\text{Thus, } \text{Log. } F_n = \text{Log. } 30 + an + bn^2 + cn^3$$

It is unnecessary to employ powers of n higher than the cube, because their coefficients would be insensible, as the calculation will show. To determine the coefficients $a b c$, he makes use of the elastic forces, observed at the temperatures on the centigrade scale of 100° , 75° , 50° , and 25° ; whence result these conditions,

$$\begin{array}{ll}
 n = 0 & F = 30.00 \text{ inches} \\
 n = 25 & F_{25} = 11.25 \\
 n = 50 & F_{50} = 3.50 \\
 n = 75 & F_{75} = 0.910
 \end{array}$$

Substituting these conditions in the above general formula, and bearing in mind that the logarithm of a fraction is equal to the logarithm of the numerator minus the logarithm of the denominator, we have the three following equations of conditions.

$$-0.4259687 = 25.a + 625.b + 15625.c.$$

$$-0.9330519 = 50.a + 2500.b + 125000.c.$$

$$-1.5180799 = 75.a + 5625.b + 421875.c.$$

Doubling the first, and subtracting it from the second, a disappears; trebling it, and subtracting it from the third, a also disappears. Then dividing each of the resulting equations by the coefficient of b , we have

$$-0.00006489160 = b + 75.c.$$

$$-0.00006404635 = b + 100.c.$$

Subtracting the one of these from the other, b will disappear; and dividing it by the coefficient of c , we shall have c . Next, by substituting the value of c in one of these equations, we get b . Lastly, putting b and c in one of the two first equations, we have a . Thus we find

$$a = -0.01537419550$$

$$b = -0.00006742735$$

$$c = +0.00000003381$$

Whence the whole formula $\text{Log. } F_n = \text{Log. } 30 + an + bn^2 + cn^3$ is completely determined, and may serve for calculating F_n , relative to any proposed value of n .

If we make, for example, $n = 100$, we shall have the elastic force at 100 degrees below the boiling point, or at the temperature of melting ice. We thus obtain

$$\text{Log. } F_n = 1.4771213 - 2.1778831 = -0.7007618.$$

Or employing negative indices in order to make use of the ordinary logarithmic tables,

$$\text{Log. } F_n = \bar{1}.2992382, \text{ whence}$$

$$F_n = 0.19917 \text{ inches; and observation gives us } 0.200.$$

The error is obviously insensible; and we may adopt, says M. BIOT, our formula as representing the experiments of Mr. DALTON. To introduce the FAHRENHEIT degrees into the formula, calling them f , and counting from 212° , we have $\frac{5}{9}f = n$; and substituting this value of n in the preceding formula, we obtain

$$a = -0.00854121972$$

$$b = -0.00002081091$$

$$c = +0.00000000580,$$

whence $\text{Log. } F_f = 1.4771213 + af + bf^2 + cf^3$, f being the number of degrees of FAHRENHEIT, reckoning them from 212° , positive below and negative above this point of departure.

By the above formula, thus elaborately investigated by M. BIOT, I have computed the elastic forces of steam at the three successive temperatures of 232° , 262° and 312° , or 20° , 50° and 100° , above the boiling point of FAHRENHEIT's scale.

In the first case we have $f = -20$ and $af + bf^2 + cf^3 = 20 + 400b - 8000c$; f is negative, being above the point of departure 212° , and, consequently, the products af and cf^3 are positive, while bf^2 becomes negative.

$$\begin{aligned} 20 a &= 0.170824 \\ 400 b &= - 0.008324 \\ 8000 c &= + 0.000046 \end{aligned}$$

$$\begin{aligned} &0.162546 + \log. 30 \text{ or } 1.477121 \\ &1.477121 \end{aligned}$$

$$\text{Log. of } 43.62 = 1.639667$$

By BIOT's formula therefore at 232 F.	-	43.620
My experiments	- - -	44.700
Mr. DALTON's table	- - -	43.25
BETANCOURT	- - -	47.20

By M. POUILLET's table at the end of BIOT's 1st vol.

$$\text{computed from the above formula} \quad - \quad 43.500$$

The difference between BIOT and my experiments here is only 1.10 inches.

2d Example. Temperature 262° FAHR. $f = 50$

$$\begin{aligned} 50 a &= 0.4270609 \\ 2500 b &= - 0.0520272 \\ 125000 c &= + 0.0007250 \end{aligned}$$

$$0.3757587$$

$$\text{Log. } 30 = 1.4771213$$

$$\text{Log. of } F_{262^{\circ}} = 1.8528800 \quad F_{262^{\circ}} = 71.265$$

Experiment	74.600
DALTON's table	69.700
POUILLET's table	70.800
BETANCOURT	82.500

The disparity between BIOT's formula and experiment becomes more apparent now: it amounts to 3.335 inches.

At 266° FAHR. which corresponds to 130° centigrade, I make it from BIOT's first formula 77.053, while at 130° by M. POUILLET, it is 75.68 ;* difference 1.973. Finally,

At the temperature of 312°, $f = 100$

$$\begin{array}{rcl} 100 a & = & 0.854121972 \\ 10.000 b & = & - 0.208109100 \\ 1000000 c & = & + 0.005800000 \\ \hline & & 0.651812872 \\ & & 1.477121300 \end{array}$$

$$\text{Log. of } F_f = 2.128934172 \quad F_f = F_{100} = 134.57$$

Experiment gives 167.00

Mr. DALTON's table 125.85

The difference between experiment, and both calculations, is now excessive, and even between the two latter it amounts to nearly 9 inches.

From this ample investigation, we may legitimately conclude, that we ought to receive such geometrical representations with great caution. M. BIOT, indeed, with a candour becoming his genius, admits these formulæ to be merely tentative approximations. The high reputation of this philosopher, and the geometrical skill here displayed, might have led the scientific world to repose confidence in his formula, within the limits of $55\frac{1}{2}$ degrees centigrade = 100 FAHR. It was therefore entitled to a deliberate examination.

It is curious to observe that my very simple formula, $\text{Log. } F = \text{Log. } 28.9 \pm n. \text{ Log. } r$, gives good approximations,

* 130° centigr. gives by M. P. force of vapour = 1907.07 millimetres, of which taking 25.4 to the English inch, we have $\frac{1907.07}{25.4} = 75.08$ as above.

through a much more extensive range, than the elaborate formula of the distinguished French geometer. Even when carried so high as the 310th degree of FAHR., we have

$$\text{Log. } 28.9 + n. \log. r = 2.19810 = L, F_{100}; \text{ hence} \\ F_{100} = 157.8$$

Experiment gives 161.3, a difference of only $3\frac{1}{2}$ inches at this prodigious elasticity; which may be deemed altogether unimportant in practice.

BIOT's formula gives a result 31 inches, and Mr. DALTON's 40 in defect.

Of Professor ROBISON's higher numbers, it is merely necessary to examine the successive differences for every 10° above 212° . These are 7.2, 8.9, 10.2, 11.9, 13.5, 13.8, 11.8, and the second differences are $+ 1.7 + 1.3 + 1.7 + 1.6 + 0.3 - 2.0$.

Such striking irregularities cannot exist in the progression of nature. BETANCOURT's are liable to a similar censure. We may find indeed small discrepancies in the best observations at such temperatures.

§ II. *Experiments to determine the elastic forces of the vapours of alcohol, ether, oil of turpentine, and petroleum or naphtha.*

The determination of the elasticities of these vapours is a very interesting problem in chemical philosophy. It may possibly unfold the law which connects temperature and elastic energy, and it may furnish likewise some useful applications.

Mr. DALTON has examined the subject with considerable care.

My experiments were performed with the apparatus above described, and were verified by frequent repetitions. The following results were noted down during the progress of the experiments.

TABLE III.

Elastic forces of the vapours of alcohol, ether, oil of turpentine, and petroleum or naphtha.

Ether.		Alcohol sp. gr. 0.813		Alcohol sp. gr. 0.813.		Petroleum.	
Temp.	Force of Vapour.	Temp.	Force of Vapour.	Temp.	Force of Vapour.	Temp.	Force of Vapour.
34°	6.20	32	0.40	193°.3	46.60	316°	30.00
44	8.10	40	0.56	195.3	50.10	320	31.70
54	10.30	45	0.70	200	53.00	325	34.00
64	13.00	50	0.86	206	60.10	330	36.40
74	16.10	55	1.00	210	65.00	335	38.90
84	20.00	60	1.23	214	69.30	340	41.60
94	24.70	65	1.49	216	72.20	345	44.10
104	30.00	70	1.76	220	78.50	350	46.86
		75	2.10	225	87.50	355	50.20
2nd.	Ether.	80	2.45	230	94.10	360	53.30
105°	30.00	85	2.93	232	97.10	365	56.90
110	32.54	90	3.40	236	103.60	370	60.70
115	35.90	95	3.90	238	106.90	372	61.90
120	39.47	100	4.50	240	111.24	375	64.00
125	43.24	105	5.20	244	118.20	Oil of Turpen.	
130	47.14	110	6.00	247	122.10		
135	51.90	115	7.10	248	126.10	Temp.	Force of Vapour.
140	56.90	120	8.10	249.7	131.40	304°	30.00
145	62.10	125	9.25	250	132.30	307.6	32.60
150	67.60	130	10.60	252	138.60	310	33.50
155	73.60	135	12.15	254.3	143.70	315	35.20
160	80.30	140	13.90	258.6	151.60	320	37.06
165	86.40	145	15.95	260	155.20	322	37.80
170	92.80	150	18.00	262	161.40	326	40.20
175	99.10	155	20.30	264	166.10	330	42.10
180	108.30	160	22.60			336	45.00
185	116.10	165	25.40			340	47.30
190	124.80	170	28.30			343	49.40
195	133.70	173	30.00			347	51.70
200	142.80	178.3	33.50			350	53.80
205	151.30	180	34.73			354	56.60
210	166.00	182.3	36.40			357	58.70
		185.3	39.90			360	60.80
		190	43.20			362	62.40

Remarks on the preceding table.

The ether of the shops as prepared by the eminent London apothecaries, boils generally at 112° ; but when washed with water, or re-distilled, it boils at 104° or 105° . It may by rectification, however, be made to boil at a still lower temperature.

Concerning the boiling point of oil of turpentine, curious (may we say ridiculous) discrepancies exist in our systems of chemistry. Dr. MURRAY, for example, in the table of the scale of temperature at the end of the first volume of his valuable system, last edition, places the boiling point of oil of turpentine at 560° . Mr. DALTON, vol. 1. p. 39. of his new system of chemical philosophy, says "several authors have it that oil of turpentine boils at 560° . I do not know how the mistake originated, but it boils below 212° , like the rest of the essential oils." I made with much care several experiments on this point, previous to ascertaining the force of its vapour, and found its boiling point to be about 316° . When recently distilled, however, it will boil at 305° . Did it boil below, or even at 212° , as Mr. DALTON asserts, *then*, long before the included portion in the above experiments had reached the 304th degree, it would have acquired such an elasticity as to support a high column of mercury, instead of being barely *in equilibrio* with the atmospheric pressure.

Plunge a phial half filled with fresh oil of turpentine into a metal cup containing any fixed oil. Heat the cup gradually. It will be found that, at the temperature of 316° , the oil remains in steady ebullition, as indicated by a thermometer suspended in the centre of the phial. Prior to this, even at

212°, some small bubbles will be evolved, principally owing to the moisture dispersed in the pores of the oil, from the water originally mixed with the crude turpentine in its distillation. If the heat be very rapidly thrown in, while the upper surface of the oil of turpentine has the area only of a one or two ounce phial, it is possible to heat it to 360° or 370°, in apparent contradiction to the theory of latent heat; for when a liquid boils in an open vessel, according to Dr. BLACK, its temperature should remain stationary. The true cause of this phenomenon is developed towards the conclusion of this memoir. The specific caloric of the vapour of the volatile oil is so small, compared to that of water, that the heat may readily be quicker introduced than the boiling process can abstract it. Concerning the boiling point of this oil, I have since inquired of a manufacturer; and he states its boiling point at 320°. Essential oil of rosemary, when kept for some time, boils at 270°; recent oil at 212°. To assign the cause of this difference, is foreign to our present object.

The vapour of ether follows nearly the same rate of expansion as water, if we start from their respective boiling points. This was observed also in Mr. DALTON's experiments; and from this single analogy, chiefly, he laid down the general law, "that the variation in the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force."

My experiments on oil of turpentine and petroleum show the fallacy of this generalization, if we reckon the common thermometric scale a tolerably correct index of temperature; but if, with Mr. DALTON, we consider our thermometric scale,

as very erroneous, then ether itself is an exception to its own law, to use this paradoxical, though just expression. In consequence of his peculiar thermometric ideas, Mr. DALTON has abrogated the above law, which he had himself framed ; though it is curious to observe, in some respectable treatises on chemistry, both hypotheses detailed, without indicating their mutual incompatibility. M. BIOT, likewise, far from imagining that the law had been repealed for 8 or 9 years, proposes to judge by its provisions of the total elastic force of every vapour at 100° centigrade, to serve as the basis of the determination of their respective specific gravities at that temperature.*

My experiments show that from 105° to 167°.5 FAHRENHEIT, ether trebles the tension of its vapour, as water also does from 212° to 272°.7 ; both containing nearly, but by no means exactly, equal intervals of the FAHRENHEIT graduation. According to Mr. DALTON's corrected scale of temperature, we have,

$$212^{\circ} \text{ FAH.} = 212^{\circ} \text{ DALTON.} \quad 105^{\circ} \text{ FAH.} = 119^{\circ} \text{ DALTON.}$$

$$273 \text{ F.} = 256.4 \text{ D.} \quad 167.5 \text{ F.} = 176 \text{ D.}$$

real interval = $\frac{273 - 105}{212 - 119}$ by DALTON. By DALTON $\frac{167.5 - 105}{212 - 119} =$ the real interval of temperature.

Thus we see, that while the interval for trebling the tension of ethereal vapour is 57°, that for aqueous vapour is only 44°.4 ; quantities that are to each other nearly as 100 : 80. Hence, according to this eminent chemist, ether must take for

* "On peut calculer par la loi de M. DALTON, quelle doit être, pour chacun d'eux, la force élastique totale de sa vapeur à la température de 100 degrés." *Traité de Physique*, Tome i. p. 393.

trebling the force of its vapour a fifth part more heat than water does.

I hope presently to be able to adduce satisfactory experimental evidence, that our thermometric indications are not at all so unequable as Mr. DALTON conceives.

Meanwhile, in examining closely the table of the vapour of ether, a beautiful analogy with that of water presented itself. The series of ratios representing the progression of the latter being lowered a single step, will accurately fit the former. At 30 inches of elasticity 1.23 was our initial number for aqueous vapour; for ethereal, it becomes 1.22; increasing or diminishing by unity each time in the second decimal figure, according as we descend or ascend by intervals of 10° of the FAHRENHEIT scale.

The following is a general view of the results.

TABLE IV.

The observed tension of ethereal vapour compared with the ratios 1.22, 1.23, &c. and 1.22, 1.21, &c.

Temp.	Quotients.	Expert.	Temp.	Product.	Expert.
104°	—	30.00	105°	—	30.0
94	24.7	24.70	115	36.6	35.9
84	20.2	20.00	125	44.3	43.24
74	16.3	16.10	135	53.4	51.9
64	13.06	13.00	145	63.6	62.1
54	10.3	10.3	155	75.4	73.6
44	8.1	8.1	165	88.2	86.4
34	6.35	6.2	175	102.0	99.1
			185	117.3	116.1
			195	134.0	133.7
			205	151.3	151.3

The numbers derived from calculation give a surprising accordance with those observed in the lower range. In the

upper range, the correspondence is as good as the delicacy of the experiments at such temperatures could permit us to expect. The experiments have been presented without modification. I must own, that when first the above perfect coincidence appeared, it gave me no small pleasure, as it led me to suppose that I had discovered the hidden chain of nature.

In treating of the vapour of alcohol, Mr. DALTON considers it as irregular in the progress of its elastic force by heat, owing to its not being a homogeneous liquid. He suspects "that the elastic force in this case is a mixture of aqueous and alcoholic vapour." I cannot see the cogency of this argument; for, if the separate bodies have a regular progression, the mixture ought not surely to be anomalous. I believe, however, that if the experiments were made with due accuracy, alcohol would be found as methodical in the elastic march of its vapour as other bodies. The following table will afford satisfactory proofs of the justness of these views. For absolute alcohol, the progression is probably as simple as that of the preceding vapours. But for alcohol, sp. gr. 0.813, which though highly rectified, contains not a little water, we should expect it to result from a composition or modification of ratios. After some search on this principle, I accordingly found it. Starting from the boiling point 174° , or for the convenience of comparison with the table, from the decade 170° , we move not by a unit, as before, but by a unit and a tenth; or the initial ratio 1.26 is affected at each step or term of 10° , with the number ± 0.011 , the signs being employed as in the preceding cases.

TABLE V.

Elastic force of the vapour of alcohol compared with the ratios.

Temp.	Calculated.	Observed.	Temp.	Calculated.	Observed.	Temp.	Calculated.	Observed.
250°	130.24	132.3	170°	28.3	28.3	90°	3.41	3.4
240	111.13	111.24	160	22.46	22.6	80	2.52	2.45
230	93.94	94.1	150	17.7	18.0	70	1.85	1.76
220	78.67	78.5	140	13.8	13.9	60	1.35	1.23
210	65.29	65.0	130	10.65	10.6	50	0.97	0.86
200	53.69	53.0	120	8.16	8.10	40	0.69	0.56
190	43.76	43.2	110	6.2	6.00	30	0.49	0.38
180	35.35	34.73	100	4.67	4.50			

$$\frac{28.3}{1.26} = 22.46 \therefore 28.3 \times \frac{1.26 - .011}{1.26} = 35.35$$

$$28.3 \times \frac{1.26 - .011}{1.26} \times \frac{1.26 - .022}{1.26} = 43.76 \text{ \&c.}$$

$$\frac{22.46}{1.271} = 17.7, \text{ \&c.}$$

The correspondence here exhibited between the observed and calculated elasticities is remarkable; nor does the difference ever exceed what would be produced by an error of 1° in the construction or reading off of the thermometer. This may fairly be deemed the limit of accuracy in such an experiment.

Oil of turpentine is regulated by the constant ratio 1.122, which converts any elastic force into that 10° above or below, multiplying as usual in the former, and dividing in the latter case. For petroleum the ratio is 1.14; it is also constant.

The following table exhibits a comparative view of theory and experiment.

TABLE VI.

Oil of Turpentine.			Petroleum.		
Temp.	Calcutat.	Observed.	Temp.	Calcutat.	Observed.
310°		33.5	320°		31.7
320	37.7	37.06	330	36.2	36.4
330	42.5	42.1	340	41.2	41.6
340	47.7	47.3	350	47.0	46.86
350	53.5	53.8	360	53.6	53.3
360	60.4	60.8	370	61.1	60.7

The whole of the preceding research is closely interwoven with a question of the first importance in chemical philosophy; what are the relative portions of temperature denoted by the graduations of our thermometric scale? Mr. DALTON regards the progressive elasticities of aqueous and ethereal vapour as affording countenance, if not support, to his thermometric innovations. He affirms, that if our instrument for measuring heat were accommodated to his doctrine, the quantity of expansion of its mercury is as the square of the temperature from its freezing point; then “the force of steam in contact with water increases *accurately* in geometrical progression to equal increments of temperature, provided these increments are measured by a thermometer of water or mercury, the scales of which are divided by the above mentioned law.”*

Were this position true, it would certainly bring a powerful analogy in aid of his theoretical views. We are now furnished with *data* to verify, or refute it. The following tables show the correspondence between that principle and experiment. In the table of aqueous vapour, the *first* column pre-

* New System, vol. i. p. 11.

sents his geometrical progression of that vapour, co-ordinate with his equal intervals of real temperature contained in the *second*. In the *third*, are the corresponding points of the common scale, as given by Mr. DALTON. To these points the elastic forces, as determined by experiment, are placed opposite in the fourth column.

Table second, for vapour of ether, is similarly arranged ; the first three columns being Mr. DALTON's ; the *fourth*, the faithful transcript of observation.

“ The force of the vapour of sulphuric ether,” says Mr. DALTON, “ in contact with liquid ether, is a geometrical progression, having a less ratio than that of water.” “ Ether, “ as manufactured in the large way, appears to be a very “ homogeneous liquid. I have purchased it in London, Edinburgh, Glasgow, and Manchester, at different times, of “ precisely the same quality in respect to its vapour.”* This shows that no exception can be made to my experiments on account of a supposed difference in the quality of the ether. From the mode of conducting my experiments, there remained always a quantity of liquid ether in contact with the vapour, a circumstance essential to accuracy in this research. The results were verified by frequent repetitions, and discover, in my opinion, the consistency of truth.

* New System, vol. i. pp, 20, 21.

TABLES VII. AND VIII.

DALTON's theory of the thermometric scale, compared with the observed temperatures and tensions of vapours.

Aqueous Vapour.				Ethereal Vapour.			
DALTON's geom. progression of elasticity.	DALTON's new scale of temperat.	FAHREN.	Observed elasticity.	DALTON's progression of elasticity.	DALTON's scale.	FAHREN.	Observed elasticity.
22.7 inch.	202°	199°	23.1 in.	6.1	32°	32°	5.81
30.0	212	212	30.0	9.16	52	46.6	8.67
39.5	222	225	39.11	13.77	72	62.55	12.60
52.0	232	238.6	50.3	20.65	92	79.84	18.40
69.0	242	252.6	64.5	31.0	112	98.50	27.2
91.0	252	266.8	81.5	46.54	132	118.50	37.7
120.0	262	281.2	103.5	69.88	152	139.9	56.8
158.	272	296.2	131.7	104.91	172	162.4	83.3
208.	282	311.5	164.8	157.5	192	186.5	118.3
				236.5	212	212	169.0

The numbers of the first and fourth column ought evidently to agree, if the theory be just. Their differences, on the contrary, are prodigiously great. At 272° of his scale, for example, equal to 296°.2 of ours, the law of progression makes the elastic force of aqueous vapour amount to 158 inches : experiment gives 131.7 ; and I am confident, that the latter cannot be in error above an inch or two. Again at 262°, equivalent to 281°.2 FAHRENHEIT, his theory gives the force of the same vapour at 120 inches ; by observation it is only 103.5. Now at this part of the scale, my result is confirmed by the concurrence of those obtained by BETANCOURT and ROBISON. I consider this demonstration complete. If we compare these very elasticities of Mr. DALTON, with the table formerly given by the same philosopher,* we shall find

* Manchester Memoirs, vol. 5.

discordances which no ingenuity can harmonize. At that time, 225° of FAHR. = 222° of the new scale, gave a force of vapour equal to 38.3; it is now $39.5 \cdot 252^{\circ}.6$ F. = 242° D. then coincided with an elasticity of 58.6 inches; above, it is 69. And finally, $281^{\circ}.2$ F. = 262° D. were opposite to 90 inches; they have become here 120. And yet no new experiments on the vapour of water have been adduced, to justify such immense alterations.

It may be said, indeed, that these changes arise merely from the substitution of one hypothesis for another; but the deviations from experiment are even more remarkable, since as 282° new scale, correspond to $311^{\circ}.5$ FAHR., the difference amounts to 43 inches, being more than one fourth of the total elastic force generated at that high temperature.

When we turn our attention to ether, we find the discrepancies, if possible, less easy to reconcile. At the temperature of 212° , for example, where the old and new scales meet for the last time, the force of its vapour by the geometrical progression exceeds that found from experiment, by the enormous quantity of 67 inches and a half; amounting to two fifths of the whole elastic force evolved.

May we venture, then, to conclude, from these multiplied comparisons, that the progressions of elasticity in vapours, taught by Mr. DALTON, are geometrical fictions, intended to quadrate with his notions concerning temperature; but not consonant with the laws or phenomena of nature?

Within a moderate compass, indeed, it is not difficult to suit the ratio of elastic force and the thermometric graduation to each other; but the prosecution of the enquiry into ranges more remote, detects the fallacy of such hypothetical adapta-

tions. My experiments on the vapours of water, alcohol and ether, seem to show, that the ratio of tension decreases in a certain progression as the temperature augments. Were the ratios 1.23, 1.22, 1.21, &c., which are seen to apply so well to aqueous vapour for a considerable range above 212° , to be adopted as representing the progressive march of nature, it would lead to the absurd conclusion, that at 240° above the boiling point, or 452° F., the farther influx of caloric would occasion a diminution of elasticity in the steam. The truth however is, that at the 312th degree, indications of a divergence begin to appear between the two lines of experiment and calculation, which had run for so long a space nearly parallel. The curve representing the expansive force of steam, I consider to be logarithmic, in which the ratios, as ordinates, continually diminish, without ever vanishing, or coming to an equality. The axis is an asymptote to the curve, as in the atmospherical logarithmic.

CHAPTER II.

On thermometric admeasurement, and the doctrine of capacity.

Before inquiring into the relative quantities of heat, contained in different vapours at the same tension, it will be proper to determine the primary and fundamental proposition concerning the measure of temperature. It is singular, that not one experimental fact has been advanced, capable of settling this question, amid the contending opinions of chemical philosophers. Mr. DALTON has, in particular, exerted all the resources of his genius and science to destroy our confidence in the thermometric scale; our sole guide in the vast

and intricate province of caloric. While I hope to be able to fix this now indeterminate point, by a new train of investigations, and consequently to prove the entire fallacy of his doctrine of temperature, the key-stone of his system of heat, I do not mean to affirm the absolute uniformity of expansion in bodies, by equal increments of that power. I think it indeed highly probable, that every species of matter, both solid and liquid, follows an increasing rate in its enlargement by caloric. Each portion that enters into a body must weaken the antagonist force, cohesion; and must therefore render more efficacious the operation of the next portion that is introduced. Let 1000 represent the cohesive attraction at the commencement; then, after receiving one increment of caloric, it will become $1000 - 1 = 999$. Since the next unit of that divellent agent will have to combat only this diminished cohesive force, it will produce an effect greater than the first, in the proportion of 1000 to 999; and so on in continued progression. That the increasing ratio is, however, greatly less than Mr. DALTON maintains, may, I think, be clearly demonstrated.

According to his table of equal increments of temperature, vol. i. p. 14, New System, we have the following intervals, corresponding to the five successive intervals of 90° on our scale.

From 32° to 122° , to 212° , to 302° , to 392° , to 482° .

Intervals by FAHR. of 90° , 90° , 90° , 90° , 90° .

True intervals by DALTON, $102^\circ.4$ $77^\circ.6$ $63^\circ.9$ $55^\circ.7$ $50^\circ.5$

The relative inequality of these intervals is deduced from Mr. DALTON's law, that "all pure homogeneous liquids, as
" water and mercury, expand from the point of their conge-

"tion, or greatest density, a quantity always as the square of the temperature from that point." He regards the law as resulting from the constitution of liquids, and therefore not applicable to solid bodies. This is indeed implied in its enunciation. In p. 43, after assigning reasons, he states, "that for all practical purposes we may adopt the notion of the equable expansion of solids."

Now I am prepared to prove, either, that the expansion of solids partakes of the above inequability of liquids, which nobody imagines, and for which no reason, even hypothetical, can be assigned ; or, which is the only alternative, that homogeneous solids, and mercury, proceed almost exactly, *pari passu*, in their rates of expansion by heat.

The experiments which justify this assertion were made by me about five years ago, and were then exhibited to many of my chemical friends, as also in my public lectures ; but a wish to render the series more complete, has induced me to withhold them from the public eye, till requisite leisure could be afforded for this purpose. They were performed with a pyrometer of peculiar construction, in an oblong trough filled with melting ice : a strong bar of Swedish iron was placed, from which projected at right angles, four inflexible iron arms, attachable by powerful screws to any part of the bar. The arms nearest the extremities of the bar, carried each a fine micrometer microscope, made by that admirable artist Mr. TROUGHTON. The other two arms were incurvated downwards at their extremities, which supported a metallic or other rod. This was fixed by two pinching screws at one end, but lay loose on a friction roller at the other. The loose end bore an elevated index. The curvature of these two arms was

such as to allow their extremities, with the attached rod, to be plunged *beneath the surface of oil or water, about an inch*, contained in a copper trough. This was placed parallel to the large trough, and a few inches distant from it.

The copper vessel was slowly and equably heated, by a series of argand lamps placed beneath. One micrometer watched a point projecting from the arm that held the fixed extremity of the rod. The oil was carefully agitated during the application of the heat; and the bulbs of three thermometers, mutually comparable, were immersed into it at regular distances. The micrometers were screened from the influence of the heat. They rendered the $\frac{1}{20000}$ of an inch discernible, and even a smaller quantity, by an experienced eye.

A rod of pure Swedish iron, or of such pure copper as jewellers use for alloying gold, being adjusted to the apparatus, the point on the micrometer scale, that appeared a tangent to the small luminous aperture in the thin index plate of steel, was noted down, when the liquid in the trough was at 32° . The value and truth of the micrometrical indications had been previously ascertained, by viewing through the microscopes a given surface or aperture, moved laterally, so as to make its image successively coincide with the different points of the interior notched scale.

Heat being now applied, the progressive march of the index across the field of view of the micrometer microscope was closely observed, and its position written down at intervals of 10° or 20° of the FAHR. thermometer. But as the pyrometrical details will appear in a separate memoir on the expansions of bodies, I shall state here merely what concerns the present subject.

If we denominate the absolute elongation of the heated metallic rod from 32° to 122° , 10, then its elongation from 122° to 212° ; from 212° to 302° ; from 302° to 392° ; from 392° to 482° , was in each successive interval of 90° F, as nearly as possible 10 also. The slight irregularities, incident to all delicate experimental investigations, being often in opposite directions, in different repetitions of the same experiment; or those which manifested themselves in the ascending or elongating range, were neutralized, so to speak, by others of an inverse nature, which appeared in the cooling retrocession. *Here*, the movements of the liquid mercury and of the solid rod by heat proceeded, *pari passu*, through a very great extent of temperature. Let us now recollect that these 5 increments, which on our thermometer are equivalent to $5 \times 90^{\circ} = 450^{\circ}$; and which altogether produce five times the elongation that the first interval occasions, constitute, on Mr. DALTON's scale, only 350° . If we call the first interval given by this philosopher 1.00, then the four succeeding intervals contain a range of temperature on his scheme, of only two and a half times the first; and therefore only two and a half times additional elongation should have been produced, instead of four times, as found by experiment. "Since for all practical purposes uniform increments of bulk, or expansions of *solids* by heat, correspond to uniform increments of this power;" then each of our old successive intervals of 90° may, for all practical purposes, be held to correspond to equal increments of temperature.

Mr. DALTON's intervals from 32° to 482° FAHR. are as before given, $109^{\circ}.4$; $77^{\circ}.6$; $63^{\circ}.9$; $55^{\circ}.7$; $50^{\circ}.5 = 350^{\circ}.1$. Now, if we call the first quantity 1.00, it will produce on a

metallic rod a corresponding effect in expansion = 1.00. The next interval of Mr. DALTON's scale (equal always to 90° FAHR.) can produce only $\frac{3}{4}$ of the effect of the first, or as 75 to 100. The third, fourth, and fifth intervals will give the fractional expansions in reference to the first, of $\frac{63}{100}$, $\frac{54}{100}$, and about $\frac{50}{100}$, or merely a half.

No such diminution of effect was observed in the experiments; from 392° to 482° F., the rod elongated as much as from 32° to 122°, or double the quantity compatible with the DALTONIAN hypothesis. Thus therefore we have a rigid, and I think unanswerable demonstration of the general correctness of the common scale of temperature, and of the extreme inaccuracy and inapplicability of Mr. DALTON's geometrical substitute. Should the preceding statement leave any doubt or obscurity concerning the legitimacy of the inference now drawn, I trust it will be entirely removed, when the details of the experiments are published, with drawings of the apparatus, in my treatise on pyrometry.

Yet though the mercury in the thermometer tube move, *pari passu*, with a metallic rod, deemed uniform in its expansion, it does not prove perfectly equal uniformity of expansion to belong to the mercury. It will seem, no doubt, a paradoxical assertion, that of two bodies marching together, hand in hand, one of them may have an equable pace, while that of the other is regularly, but very slowly accelerated. Yet I think the position just. It proceeds from a circumstance in the thermometer sufficiently obvious, but which seems to have escaped our *systematic* writers. I do not rest the proposition on any imperfection of workmanship, or supposed irregularity in the expansions of the glass.

Let us take a thermometer, the calibre of whose stem is perfectly uniform, and whose scale is exactly divided. Let it have a range from zero to the 656th degree, at which mercury boils, by the accurate experiments of CREIGHTON. At 32° , let the mercury stand at the bottom of the ivory scale, where of course the graduations commence. The bare part of the instrument is consequently the plunging limit, in most chemical researches on the temperature of liquids. Immerse the bulb in common oil, or oil of vitriol heated to 212° ; $\frac{1}{63}$ * part of the whole included mercury, will now ascend above that part of the stem plunged in the liquid. The part actually exposed to the heat, and by whose expansion the column on the scale is supported, is only $\frac{62}{63}$ of the initial mass. Augment the heat of the oil till the instrument indicate 392° ; we know that there remains now, under the immediate influence of the heat, $\frac{61}{63}$ nearly of the original weight of mercury; and finally, at 572° , only about $\frac{60}{63}$ rest in the immersed part of the stem and bulb.

$\frac{2}{63}$ or $\frac{1}{21}$ parts may be considered as no longer subjected to the power of caloric. If the thermometer stem were recurved near the bulb, the mercury in the stem placed horizontally would be cold; and this proposition would be almost exactly true.

Now, since the calibre and divisions are uniform, the capacity of the tube from the point marked 212° , to that marked 392° ; and again from this, to that opposite to 572° , is in each equal to its capacity from 32° to 212° . Hence these three equal capacities are filled by the expansions of the three unequal quantities of mercury 62, 61, 60. At the

* A minute fraction less; but we need not complicate the statement with it.

highest station, the column of quicksilver equal on the stem to $3 \times 180^\circ$, is sustained by the expansion of 60 parts; at the middle point, $2 \times 180^\circ$ is supported by that of 61; and at 212° there are 62 parts of mercury to sustain 180° in the tube. Or, to put it in another form, these three successive spaces on the scale are equal; the first portion of mercury is protruded into it by the expansion of 62 parts in the bulb; the second portion by the expansion of 61; and the third by that of 60.

Therefore, if these three thermometric intervals of 180° , each of which holds an equal measure of mercury, contain also equal increments of temperature, as denoted by the equal increments of a metallic rod; then, these three equal effects are produced from the unequal quantities of mercury 62, 61, 60. This liquid, then, must have an increasing rate of expansion, the inverse of these numbers, for every 180° of the scale, or $\frac{1}{62}$, $\frac{1}{61}$, $\frac{1}{60}$. That is to say, 60 parts at 572° do the same work by the same power of caloric, as 61 at 392° , and 62 at 212° .

I believe this to be the real nature of mercurial expansion, and the true condition of the thermometer; which is an equable measurer of heat, because the mercury possesses the above increasing rate of expansion. Were the mercury, on the contrary, absolutely uniform in its augmentations of volume by equal increments of heat, then for an instrument whose bulb alone in practice can be immersed, the three above ranges should have the corresponding parts of the scale shortened in the successive proportions of 62 to 63; 61 to 63; and 60 to 63; quantities taken together nearly equal to 9° , or $= 3 \times \frac{1}{62} \times 180 = \frac{440}{62}$.

Whatever reception these speculations may experience, they must not be confounded with the experiments on the expansions of metallic rods, and the corollaries, which have a distinct and independent existence.

§ II. *On the doctrines of capacity, as connected with the preceding investigation.*

Dr. CRAWFORD and DE LUC, tried to verify the justness of the thermometric indications, by mixing together water at 212° and 32° ; when the former found 122° , and the latter 119° , to be the resulting temperature. DE LUC's number is 3° below the mean; Dr. CRAWFORD's is exact. This ingenious philosopher afterwards sought to confirm the evidence thus given to the accuracy of the scale, by other experiments, which were however of rather an equivocal import. Both of the above results have been condemned and rejected by Mr. DALTON: he states the true mean temperature to be not 122° , nor even 119° , but 110° . For this deviation, the reasons which he assigns appear, independently of all arguments derived from other quarters, to be in themselves inconclusive. He says, "the temperature of the above mixture ought to be found above the mean 122° ." "Water of these two temperatures (32° and 212°) being mixed, loses about $\frac{1}{90}$ of its bulk. This condensation of its volume* must expel a quantity of heat, and raise the temperature above the mean." p. 7. Again, p. 50, "that water increases in its capacity for heat with the increase of temperature, I consider demonstrable from the following arguments. 1st. A

* That condensation of volume in a liquid, is no proof of the expulsion of heat, is shown in my Essay on Sulphuric Acid.

“ measure of water at any one temperature mixed with a
“ measure at any other temperature, the mixture is less than
“ two measures. Now, a condensation of volume is a certain
“ mark of diminution of capacity and increase of temperature,*
“ as in the mixture of sulphuric acid and water; or the effects
“ of mechanical pressure, as with elastic fluids. Second,
“ when the same body suddenly changes its capacity by a
“ change of form, it is always from a less to a greater as the
“ temperature ascends; for instance, ice, water and vapour.
“ Third, Dr. CRAWFORD acknowledges from his own ex-
“ perience, that dilute sulphuric acid, and most other liquids
“ he tried, he found to increase in their capacity for heat with
“ the increase of temperature. Admitting the force of these
“ arguments, it follows, that when water of 32° and 212° are
“ mixed, and give a temperature denoted by 119° of the
“ common thermometer, we must conclude that the true mean
“ temperature is somewhere below that degree. I have
“ already assigned the reason why I place the mean at 110° .”
Now the only reason I can elsewhere find, is derived from his
general law, “ that all homogeneous liquids expand, as the
square of the temperature, from the point of greatest density
or congelation.” In p. 7, he ventures to assert nothing more
than, “ that it is not improbable that the true mean tempe-
“ rature between 32° and 212° , *may* be as low as 110° FAH-
“ RENHEIT.”

Satisfied from my pyrometrical experiments, that his
general hypothesis of the expansion of liquids being as the

* For the entire fallacy of this reasoning, see my Essay just quoted; expansion of
volume should by Mr. D. increase capacity and diminish temperature. The very
reverse is shown in that paper.

square of their temperature, is totally inapplicable to mercury, the inference relative to the thermometric mean between 32° and 212° cannot be allowed. But let us examine, on their own merits, the preceding arguments against Dr. CRAWFORD and DE LUC's verification of the mean temperature between that of freezing and boiling water.

The reasoning derives its sole force from the assumption, that the capacity of water for heat, increases as its temperature is raised. There is adduced, however, no fact in the least decisive on this main point. What analogy is there between the entire change of form and constitution suffered by an incompressible liquid, on becoming an elastic vapour, and the progressive heating of the liquid itself? Or, although dilute sulphuric acid and other liquids should increase in their specific caloric on being heated, which however has not been satisfactorily demonstrated, are we to assert that water must do so too? It is a matter of surprise to me, that a philosopher of Mr. DALTON's judgement and acuteness should have pressed such inconclusive analogies into his service. He knew well that water is endowed with some curious peculiarities, when compared with other liquids, or anomalies, as we idly stile them; for they constitute no anomaly in nature, but wisely fit water for performing the important functions assigned to it in the economy of our globe.

In a series of experiments, carefully conducted on the relative capacities for heat, of water, sulphuric acid, oil of turpentine, and spermaceti oil, published in my Essay on hydrochloric acid and the chlorides; it seems to be directly demonstrated that the specific heat of water does not *increase*, but actually *diminishes*, and that very conspicuously, as its

temperature rises. It is there proved, that from 210° to 150° FAHR. the specific heat of oil is to that of water as 597 to 1000; and from 150° to 90° as 513 to 1000. The same proportional difference of relation is exhibited by the other two liquids. Now, were the phenomenon occasioned by the oil of vitriol, common oil, and oil of turpentine, increasing in *their* capacities for heat in a still more rapid ratio than water, we should undoubtedly expect, from the innate differences between the specific heats of these three substances, to find that they would move independently on each other, or at different rates. But *their* uniform advance together, while water alone varies in this respect, shows distinctly, that in the water resides the cause of the variation. This reasoning may be illustrated in many ways, but by nothing more clearly than the exploded astronomical system of the diurnal and annual movements of the sun and fixed stars; in support of which, very extravagant hypotheses had to be contrived.

The single fact of the motion of the earth once admitted, reduced the PTOLEMAIC chaos to order. If, in like manner, we should suppose an increasing ratio in the specific heat of water, then we must also suppose a much more rapid increase in the ratios of the above three substances, although their individual specific heats are greatly inferior to that of water. Ought not that body, which has of all others the most decided relation to heat, or highest specific heat, to have also its ratio most decidedly or rapidly augmented? In adopting the increasing specific heat of water, we must farther assume, that, however different the initial specific heats of the above three liquids may be, yet, while they possess all the same rate of increase, water alone has a different one; an inadmissible supposition. All

these difficulties and contradictions are removed at once by the experimental fact, that water is endowed with a decreasing ratio in its capacity for caloric, as its temperature is augmented.

Since finishing the above researches on specific heat, I have been led to examine attentively the systematic accounts of this subject in our chemical treatises; and I find that BERTHOLLET, with a sagacity peculiar to himself, had anticipated, from the chemical constitution of bodies, such an experimental result as I have recently obtained; though the statements then prevalent all militated against his views. "If caloric obey the usual laws of attraction, when it is in small quantity, relative to the body to which it is united, it will enter into more intimate combination; and hence the elasticity or expansive energy of it, on which temperature depends, may be overcome, and a larger quantity be required to produce a given temperature. Hence, the quantity of caloric contained in bodies in the first stage of temperature, may be greater than it will be higher in the scale."

In the Essay above referred to, I have shown that this circumstance in water, renders it peculiarly qualified for serving as the magazine and equalizer of the temperature of the globe. Since at our ordinary atmospherical heats, it possesses the greatest capacity for caloric, small variations in its temperature give it a great modifying power over the circumambient air. Although the doctrine of final causes be no safe guide to the discovery of unknown truths, yet when it concurs with experiment, we may deem it an agreeable confirmation. This is finely illustrated by Count RUMFORD's speculations on the maximum density of water being placed

several degrees above its point of congelation ; a fact which does not hold with regard to any other homogeneous liquid.

If the specific heat of water, then, diminish as its temperature advances from the freezing to the boiling point, an interval of 10° near 32° , will contain more caloric than ten degrees near 122° , and still more than the same intervals near 212° . On this principle we can readily account for the results obtained by Mr. DALTON, in mixing with water at different temperatures a known proportion of ice ; though it is remarkable that this able chemist did not see in them any thing inconsistent with his own opposite views upon specific heat.

“ $176^{\circ}.5$ expresses the number of degrees of temperature, such as are found between 200° and 212° of the old or common scale, entering into ice of 32° , to convert it into water of 32° ; 150° of the same scale suffice, he says, for the same effect, between 122° and 130° : and between 45° and 50° , 128° are “ adequate to the conversion of the same ice into water. These three resulting numbers (128 , 150 , 176.5) are nearly as 5 , 6 , 7 . Hence it follows, that as much heat is necessary to raise water 5° in the lower part of the old scale, as is required to raise it 7° in the higher, and 6° in the middle.”*

Mr. DALTON, instead of adopting the obvious conclusion, that the capacity of water for heat is greater at lower than it is at higher temperatures, and that therefore a smaller number of degrees of the former should melt as much ice as a greater number of the latter, ascribes the deviation denoted by these numbers, or their differences, to the gross errors of our thermometric graduation ; which he considers

so excessive, as not only to equal, but greatly to overbalance the real increase in the specific heat of water ; which left to its own operation, would have produced opposite experimental results.

That our thermometric scale has no such prodigious deviation from truth, or uniformity of indication, I conceive to be fully established, and therefore the only legitimate inference from these very experiments of Mr. DALTON, is the *decreasing* capacity of water with the increase of its temperature.

It deserves to be remarked, that my experiments on the relative times of cooling a globe of glass, successively filled with water, oil of vitriol, common oil, &c. give exactly the same results as Mr. DALTON derived from mixtures of 2 ounces of ice and 60 of water. This concurrence is the more satisfactory, since, when the Essay on hydrochloric acid was written, I had no recollection of Mr. DALTON's experiments. I found that from 210° to 150° the specific heat of oil bears to that of water the ratio of 597 to 1000 ; and from 150° to 90° , that of 513 to 1000. Now, at his highest and middle temperatures of 200° and 120° , which come nearest to mine of 180° and 120° , we have by him the ratio of $176^{\circ}.5$ to 150° .

But $597 : 513 :: 176 : 150$ exactly, which is a very striking coincidence, and affords the happiest confirmation of the accuracy of both sets of experiments, as well as of the justness of the principles on which they were conducted, and on which, particularly, my reductions were founded. We now see the reason why, when equal weights of water at 32° and 212° are mixed, the temperature may be *below* the mean, as was found by DE LUC. The capacity at the middle tempe-

perature, is greater than the mean capacity of the two extremes, (that is of the ingredients mixed, ice, cold and boiling water) and therefore the thermometric tension will be lessened, and its mercury will descend on the scale.* This diminution of temperature will cause a corresponding diminution of bulk, which affords a complete answer to Mr. DALTON's first and only plausible argument, formerly quoted against Dr. CRAWFORD's deductions, and the opinions of DE LUC. With regard, however, to these experiments, of mixing hot and cold water to find a mean temperature, there are sufficient difficulties to render the result uncertain to 2 or 3 degrees. Hence, nothing of moment can safely be inferred from them.

Concerning sulphuric acid in its various states of dilution, I beg to refer the reader to my Essay on the subject, where he will find several peculiarities relative to its volume at different acid strengths, that entirely change its relations to caloric. I have not seen these formerly adverted to by any chemist. They were evidently unknown to Mr. DALTON.

CHAPTER III.

On the latent heat of different vapours.

What relation is there between the caloric existing in the vapours of different substances, and the temperatures at which they respectively acquire the same elastic force?

On this subject I am not acquainted with any preceding inquiries, though a question of such interest has probably not escaped examination.

* Taking Mr. DALTON's three numbers as correct: then $\frac{176.5 + 128}{2} = 152.25$. But 150° in the middle are equal to the former mean of the two. Hence, the proposition in the text is demonstrated.

In this research I employed a very simple apparatus ; and with proper management, I believe it capable of giving the absolute quantities of latent heat in different vapours, as exactly as more refined and complicated mechanisms. At any rate, it will afford comparative results with great precision.

It consisted of a glass retort of very small dimensions, with a short neck inserted into a globular receiver of very thin glass, and about three inches in diameter. The globe was surrounded with a certain quantity of water at a known temperature, contained in a glass basin. 200 grains of the liquid, whose vapour was to be examined, were introduced into the retort, and rapidly distilled into the globe by the heat of an argand lamp. The temperature of the air was 45° , that of the water in the basin from 42° to 43° , and the rise of temperature, occasioned by the condensation of the vapour, never exceeded that of the atmosphere by 4 degrees. By these means, as the communication of heat is very slow between bodies which differ little in temperature, I found, that the air could exercise no perceptible influence on the water in the basin during the experiment, which was always completed in 5 or 6 minutes. A thermometer of great delicacy was continually moved through the water ; and its indications were read off, by the aid of a lens, to small fractions of a degree.

In all the early experiments of Dr. BLACK on the latent heat of common steam, the neglect of the above precautions introduced material errors into the estimate. Hence that distinguished philosopher found the latent heat of steam to be no more than 800° or 810° . Mr. WATT afterwards deter-

mined it more nearly from 900° to 950° ; and LAVOISIER and LA PLACE have made it 1000° .

It is evident that whenever the water, into which the latent heat is evolved by condensation of the vapour, becomes much hotter than the surrounding air, it will be impossible to ascertain how much of the caloric is dissipated; and consequently, the true quantity contained in the vapour must remain uncertain.

The sources of error in operating with the calorimeter of LAVOISIER and LA PLACE, were first pointed out by Mr. WEDGWOOD, and have been since commented on by Dr. THOMSON, and other good systematists. It is said to be difficult to obtain precisely uniform quantities of liquified ice in the repetition of the same experiments, with that celebrated apparatus.

From the smallness of the retort in my mode of proceeding, the shortness of the neck, and its thorough insertion into the globe, we prevent condensation by the air *in transitu*; while the surface of the globe and the mass of water being great, relative to the quantity of vapour employed, the heat is entirely transferred to the refrigeratory, where it is allowed to remain, without apparent diminution, for a few minutes. In numerous repetitions of the same experiment, the accordances were excellent. The following table contains the mean results. The water in the basin weighed in each case 32340 gr. The globe was held steadily in the centre of the water by a slender ring fixed round its neck. The distillation was rapidly performed, so that all the condensation took place in the globe.

Table of experimental results on the latent heat of different vapours.

200 gr. of water distilled, raised	32340 gr.		
water from	-	-	42°.5 to 49°
200 gr. alcohol, spec. gravity 0.825	-		42 to 45
200 gr. sulphuric ether ; boiling point 112°			42 to 44
200 gr. oil of turpentine	-	-	42 to 43.5
200 gr. petroleum	-	-	42.5 to 44
200 gr. nitric acid. spec. grav. 1.494. boiling point 165°	-	-	42 to 45.5
200 gr. liquid ammonia. sp. gr. 0.978	-		42 to 47.5
200 gr. vinegar. sp. gr. 1.007	-		42.5 to 48.5

Calculation from the above table of the specific or latent heats of the vapours.

1st, Water. $\frac{32340}{200} = 161.7 =$ the number of grains of water contained in the refrigeratory to one of steam. This proportion is constant for all the vapours.

From 42°.5 to 212° there are 169°.5; one half of which $= 84°.75$, or in round numbers 84°, is the rise of temperature which would be produced by adding to water at 42°.5 its own weight of boiling water; and $\frac{84}{161.7} = 0.52$, is the elevation which 200 gr. would occasion on 32340 grains.

The water was however in reality heated $6\frac{1}{2}$ degrees, or from 42°.5 to 49°. The difference, $6°.5 - 0°.52 = 5°.98$, shows the quantity of heat added to each of the 161.7 parts beyond what the same weight of boiling water would have communicated.

And $5.98 \times 161.7 = 967^\circ$, being the latent heat of the steam of water.

2d. Alcohol. Boiling point 175° . Specific gravity 0.825.

$\frac{175 - 42 \times 66.5}{2 \times 161.7} = 0.41$. 0.41 multiplied into the specific heat of liquid alcohol 0.65, is $0^\circ.266$, which represents the elevation of temperature produced by adding 200 gr. of boiling hot alcohol to 32340 gr. of water. The thermometer in the experiment rose 3° . $3^\circ - 0^\circ.266 = 2.734$, $2.734 \times 161.7 = 442^\circ$ = the latent heat of alcoholic vapour in equilibrio with the atmospheric pressure. By a similar process of calculation the latent heat of the other vapours was determined.

General table of latent heat of vapours.

Vapour of water at its boiling point			967°
alcohol	-	-	442
ether	-	-	302.379
petroleum	-	-	177.87
oil of turpentine		-	177.87
nitric acid	-		531.99
liquid ammonia		-	837.28
vinegar	-		875.00

From the phenomena exhibited in the mechanical condensation and rarefaction of gases and vapours, as well as from their general constitution, it may be inferred, that an intimate and necessary connection subsists between their latent heat, elastic force, and specific weight or density.

Hence, when their tension is the same, it appears reasonable to suppose that the product of their densities into their quantities of latent heat will also be the same. Repulsive energy

will be proportional to the quantity of heat, the repulsive power condensed or contained in a given space. Thus if the *space* left for its interposition or lodgment be in one vapour a half or a third of the amount of the space in another, we ought to find equal tension produced in the former case, by a half or a third of the latent heat required for the latter.

As the principle, I have reason to suppose, is somewhat new, let us illustrate it by an application to the three vapours in the above list which are most homogeneous, or at any rate best understood; those of alcohol, ether, and water.

Aqueous vapour of an elastic force balancing the atmospheric pressure has a specific gravity, compared to air, by the accurate experiments of GAY LUSSAC, of 10 to 16.

For facility of comparison let us call the steam of water unity, or 1.00; then the specific gravity of the vapour of pure ether is 4.00, while the specific gravity of the vapour of absolute alcohol is 2.60.

But the vapour of ether, whose boiling point is not 100°, but 112°, like the above ether, contains some alcohol; hence, we must accordingly diminish a little the specific gravity of its vapour.

It will then become instead of 4.00	-	-	3.55
Alcohol of 0.825 sp. gr. contains much water;			
specific gravity of its vapour	-	-	2.30
That of water as before, unity	-	-	1.00

The interstitial spaces in these three vapours will therefore be inversely as these numbers, or

$$\frac{1}{355} \text{ for ether; } \frac{1}{230} \text{ for alcohol; } \frac{1}{100} \text{ for water.}$$

Hence $\frac{1}{355}$ of latent heat, existing in ethereal vapour, will occupy a proportional space, be equally condensed or possess the same tension with $\frac{1}{230}$ in alcoholic, and $\frac{1}{100}$ in aqueous vapour.

A small modification will no doubt be introduced by the difference of the thermometric tensions, or sensible heats, under the same elastic force. Common steam, for example, may be considered as deriving its total elastic energy from the latent heat multiplied into the specific gravity + the thermometric tension.

Hence the elastic force of water, or

$$E_w = 970^\circ \times 1.00 + 212^\circ = 1182$$

$$E_e = 302^\circ \times 3.55 + 112^\circ = 1184$$

$$E_{al} = 440^\circ \times 2.30 + 175^\circ = 1185$$

Three equations which yield, according to my general proposition, equal quantities; or of which the differences are inconsiderable, and undeserving of notice.

Neither the specific heats nor specific gravities of the other vapours are ascertained with sufficient precision to enable us to subject them to calculation.

General equation $F - \frac{L}{D} + T = 0$: L , latent heat, D , density, T , temperature corresponding to F .

When the elastic forces of vapours are doubled, or when they sustain a double pressure, their interstices are proportionally diminished. We may consider them now as in the condition of vapours possessed of greater specific gravities. Hence, the second portion of heat introduced to give double the

elastic force need not be equal to the first, in order to produce the double tension. This view now given accords with the experiments of Mr. WATT, alluded to in the beginning of this memoir. He found that "the latent heat of steam is less when it is produced under a greater pressure or in a more dense state; and greater when it is produced under a less pressure or in a less dense state."*

BERTHOLLET thinks this fact so unaccountable, that he has been willing to discard it altogether. Whether the view which I have just opened, of the relation subsisting between the elastic force, density, and latent heat of different vapours, harmonize with chemical phenomena in general, I leave to others to determine. It certainly agrees with that *unaccountable* fact. Whatever be the fate of the investigation of the general law now respectfully offered, the statement of Mr. WATT may be implicitly received under the sanction of his acknowledged sagacity and candour.

CONCLUSION.

To the theory of latent heat, which, like the hydrostatic paradox of Archimedes, might have remained for ages a barren, though beautiful proposition, the fertile genius of that philosopher gave all at once its noblest application, and most beneficial influence on human life, by his new steam engine. After him, many minds of the first order for science and ingenuity have offered schemes of farther improvement;

* Philos. Trans. vol. 84. p. 335.

but all either frivolous or abortive; with such prophetic judgment had Mr. WATT anticipated the happiest form and structure of which it was susceptible.

Under this conviction, it is with much deference that I draw the following practical inferences from the last train of experiments.

Since the vapour of alcohol, having the same elastic force as the atmosphere, contains $\frac{4.4}{100}$ of the latent heat of ordinary steam, and since its elastic force is doubled at the 206th degree (6° below the boiling heat of water), with perhaps $\frac{1}{3}$ of additional caloric; might we not, in particular circumstances, employ this vapour for impelling the piston of a steam engine? The condensing apparatus could, I imagine, be so constructed, as to prevent any material loss of the liquid, while more than a quadruple power would be obtained from the same size of cylinder at 212° , with an expenditure of fuel not amounting to one half of what aqueous vapour consumes; or the power and fuel would be as 3 to 1, calling their relation in ordinary steam 1 to 1. A considerable engine could thus also be brought within a very moderate compass. Possibly, after a few operations of the air pump, the incondensable gas may be so effectually withdrawn, that we might be permitted to detach this mechanism, which, though essential to common engines, takes away one fourth of their power. In a distillery in this country, or on a sugar estate in the colonies, a trial of this plan might perhaps be made with advantage. While exercising its mechanical functions of grinding, mashing or squeezing the canes, it would be converting ordinary into strong spirit for rectification, or for

the convenience of carriage. Might not such an engine be executed on a small scale, for many purposes of domestic drudgery? It would unquestionably furnish a beautiful illustration in philosophy, to make one small portion of liquid, by the agency of fire, imitate the ceaseless circulation and restless activity of life.

Fig. 1.

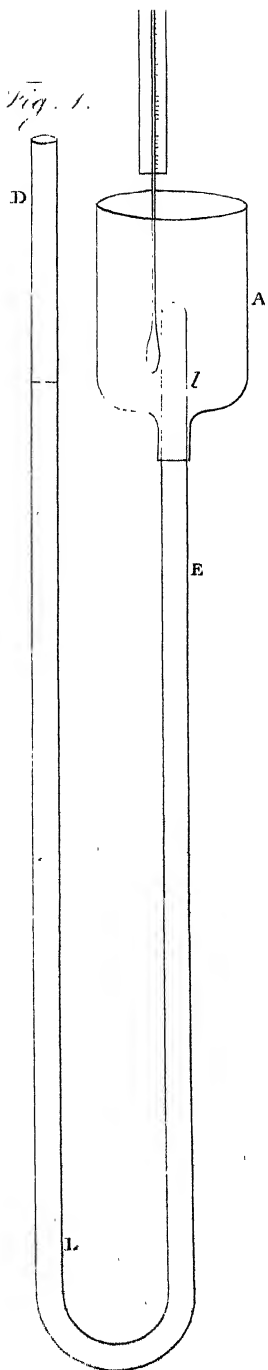
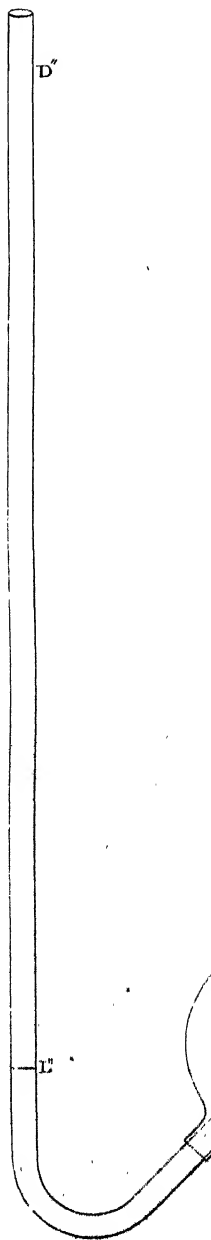
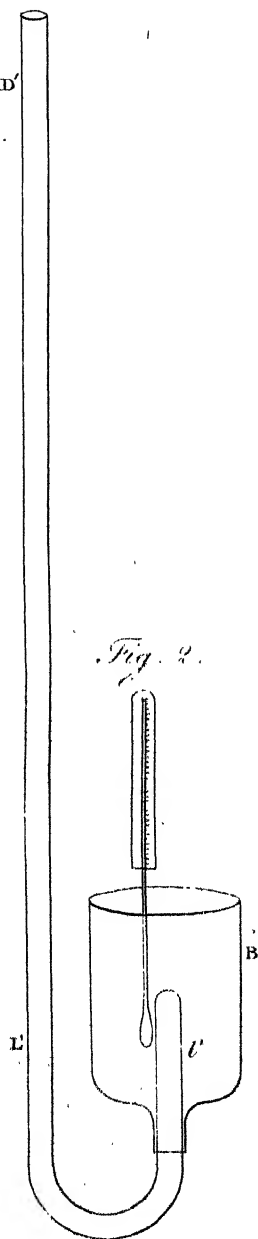


Fig. 2.



XIX. *Observations on the heights of mountains in the north of England.* By Thomas Greatorex, Esq. F. L. S. In a letter to Thomas Young, M. D. For. Sec. R. S.

Read May 7, 1818.

HAVING been desirous, for many years past, to revisit the lakes in the north of England, and wishing, when I should be able to take that tour, to make some experiments on one of the mountains in that district, I applied to the late Mr. RAMSDEN, who made for me the following instruments :

A mountain barometer.

A stationary do. (the mercury of both boiled in the tube.)

A telescope with cross-wires, and a level fitted to it, mounted on a tripod-staff.

And a small theodolite, with compass, &c.

These he assured me were made with the greatest care.

I have this summer passed some weeks in the vicinity of the Lakes ; and at Keswick I fortunately met with one of those superior self-taught geniuses, not uncommon in the North, who entered into all my views, and proved eminently serviceable to me. His name is OTLEY, and he is a watch-maker, but acts occasionally as guide up the mountains, &c.

As I wished to measure Skiddaw geometrically down to the average level of Derwent Water, I had a tapering staff made about 28 feet long, shod with an iron point, and very carefully graduated from an accurate standard yard measure, sent from the proper office, in London.

The graduation commenced from a zero about 3 feet from the bottom of the staff, so that from the zero to its top was exactly 25 feet ; the top ending in a bluntish point.

Twelve feet six inches of the upper part of the staff could occasionally be separated from the lower part, both for the convenience of carrying, and in case the wind should prevent the use of the whole length. Small cords were also attached to it, for the purpose of holding it steady and perpendicular.

We first determined the height of OTLEY's house above the lake, which was 10 yards, and at this height the stationary barometer was placed ; and Mr. CROSTHWAITE, of the Keswick Museum, undertook to note its variation and that of the thermometer every half hour.

We then ascended Skiddaw, accompanied by Mr. AIREY, a black-lead pencil maker (who proved an excellent assistant), and two boys. On the summit the barometer was set up. I first adjusted it, and privately noted down the height of the mercury ; then purposely deranged it, to let OTLEY re-adjust it, and write down his observation, which never differed from mine more than one thousandth of an inch during the whole series of observations. The heights of the attached and detached thermometers being also noted, we proceeded to the measurement by the level and staff.

The telescope being levelled, and its cross wires intersecting the highest point of the mountain, it was then pointed in the direction of the most convenient descent, and the staff carried down the hill till its top exactly coincided with the cross wires, the level of the telescope being carefully preserved ; the perpendicularity of the staff was ascertained by

plumb lines. And here I must mention how greatly we were favoured by the weather : the sun only shone during the first observation, and it was nearly a calm during the whole of the measurement, so that AIREY could frequently, by inserting the pole a little in the ground, set it on a balance, which would remain during the time necessary for adjusting the level, and observing it.

I have also to observe, that we chose so precipitous a descent, that the pole was seldom 40 feet from the telescope, therefore no allowance was necessary for the earth's curvature.

I found the most expeditious and exact mode of managing the pole, was to stop Mr. AIREY, when I perceived its top to be about an inch above the cross wires ; as I could then make signs to him to press it into the earth by little and little till the coincidence was exact : the telescope was then carried down to the pole, and placed (when levelled) in exact correspondence with the zero. The pole was again removed down to a new station, and this mode continued till we had compleated six observations, or 50 yards of descent. Here the barometer was again set up, separately examined as before ; and this process continued to the foot of the mountain.

After descending 175 yards, it was necessary to take a dead level of half a mile, which was corrected by several observations back and forward, as also by a middle station.

We measured 400 yards down the first day, and on the following day compleated 900 yards, which brought us to the village of Applethwaite, and near the level of Bristow Hill, about a mile distant.

The next day many observations were made back and

forward between Armathwaite and Bristow Hill, allowing 8 inches for the earth's curvature: a small allowance appeared necessary for refraction, the object appearing rather lower than it ought to have been. (This was in the forenoon.)

From Bristow Hill, by means of a middle station, a level was taken to Crow Park, close to the lake, and the measurement completed down to the mean height of Derwent Water.

Results of the levelling.

	yds.	ft.	in.
From the summit of Skiddaw to Applethwaite	900	0	0
Down to the level of Bristow Hill	5	0	7½
Thence to the level of Crow Park	14	2	5
Down to the average height of Derwent Water	16	0	3
Skiddaw above the lake	936	0	3½
Derwent Water above the sea at low-water mark, according to an accurate measurement by the late Mr. CROSTHWAITE	76	0	0
Height of Skiddaw by levelling	1012	0	3½

Results of the several barometrical observations on the summit of Skiddaw, Sept. 15th. 1817, at 10 hours 50 min. A. M.

Barometer below. 10 yds. above the lake	30.050. Ther. 61°.	inches.
Barometer above	27.156. Th. attached 57°. Do. detached 50°.	
Whence, by Dr. MASKELYNE's formula, the height		yds.
Measured height		= 926.1685
		= 926
	error +	— .1685
By Dr. HUTTON's formula		yds.
		= 925.285
	error —	— .715

Bar. below	50 yards down. 12 h. 20 m.	
Do. above	30.030. Ther. 62°.	
	27.310. Do. 56°.*	
Measured height		yds.
By Dr. MASKELYNE		876
		873.194477

By Dr. HUTTON	error —	2.805523
	error —	3.948

* The attached and detached thermometers did not differ for the remainder of the observations.

		100 yards down. 1 h. 10m.		
		inches.		
Bar. below	-	-	30.010. Ther. 62°.	
Do. above	-	-	27.444. Do. 56°.	yds.
		Measured height	-	826
		By Dr. MASKELYNE	-	821.71247
		By Dr. HUTTON		error — 4.28753
				error — 4.6032
		150 yards down. 1 h. 55m.		
		30.010. Ther. 63°.		
Bar. below	-	-	27.590. Do. 56°.	yds.
Do. above	-	-		776
		Measured height	-	772.3057
		By Dr. MASKELYNE	-	
		By Dr. HUTTON		error — 3.6943
				error — 4.6444
		200 yards down. 4h. 20m.		
		30. Ther. 61°.		
Bar. below	-	-	27.708. Do. 53°.	yds.
Do. above	-	-		726.
		Measured height	-	724.7245
		By Dr. MASKELYNE	-	
		By Dr. HUTTON		error — 1.2755
				error — 2.092
		250 yards down. 4h. 52m.		
		30. Ther. 60°.		
Bar. below	-	-	27.851. Do. 50°.	yds.
Do. above	-	-		676.
		Measured height	-	672.252
		By Dr. MASKELYNE	-	
		By Dr. HUTTON		error — 3.748
				error — 4.3152
		300 yards down. 5h. 50m.		
		29.990. Ther. 58°.		
Bar. below	-	-	28.002. Do. 48°.	yds.
Do. above	-	-		626.
		Measured height	-	616.731
		By Dr. MASKELYNE	-	
		By Dr. HUTTON		error — 9.269
				error — 9.6532
		350 yards down. 6h. 45m.		
		29.987. Ther. 57°.		
Bar. below	-	-	28.150. Do. 47°.	yds.
Do. above	-	-		576.
		Measured height	-	566.3645
		By Dr. MASKELYNE	-	
		By Dr. HUTTON		error — 9.6355
				error — 9.91

		400 yards down. 7h. 15m.		
		inches.		
Bar. below	-	29.980.	Ther. 55°.	
Do. above	-	28.310.	Do. 47°.	yds.
		Measured height	-	526.
		By Dr. MASKELYNE	-	513.3128
			error —	12.6872
		By Dr. HUTTON	-	12.84
		400 yards down. Sept. 6th, 1817. 10h. 15m. A. M.		
Bar. below	-	29.890.	Ther. 59°.	
Do. above	-	28.240.	Do. 54°.	yds.
		Measured height	-	526.
		By Dr. MASKELYNE	-	517.89753
			error —	8.10247
		By Dr. HUTTON	-	8.674
		450 yards down. 11h. 5m.		
Bar. below	-	29.900.	Ther. 59°.	
Do. above	-	28.396.	Do. 54°.	yds.
		Measured height	-	476.
		By Dr. MASKELYNE	-	470.2667
			error —	5.7333
		By Dr. HUTTON	-	6.2514
		500 yards down. 11h. 54m.		
Bar. below	-	29.900.	Ther. 61°.	
Do. above	-	28.564.	Do. 56°.	yds.
		Measured height	-	426.
		By MASKELYNE	-	417.8833
			error —	8.1167
		By Dr. HUTTON	-	8.65
		550 yards down. 12h. 24m.		
Bar. below	-	29.905.	Ther. 61°.	
Do. above	-	28.730.	Do. 56°.	yds.
		Measured height	-	376.
		By Dr. MASKELYNE	-	365.8427
			error —	10.1573
		By Dr. HUTTON	-	10.6186
		600 yards down. 12h. 53m.		
Bar. below	-	29.908.	Ther. 61°.	
Do. above	-	28.866.	Do. 56°.	yds.
		Measured height	-	326.
		By Dr. MASKELYNE	-	323.098758
			error —	2.901242
		By Dr. HUTTON	-	3.326

		650 yards down. zh. 5m.			
		inches.			
Bar. below	-	-	29.910. Ther. 60°.		
Do. above	-	-	29.046. Do. 56°.	yds.	
		Measured height	-	-	276.
		By Dr. MASKELYNE	-	-	266.8915
				error —	9.1085
		By Dr. HUTTON	-	-	9.446
		700 yards down. zh. 38m.			
		inches.			
Bar. below	-	-	29.910. Ther. 60°.		
Do. above	-	-	29.188. Do. 56°.	yds.	
		Measured height	-	-	226
		By Dr. MASKELYNE	-	-	221.8442
				error —	4.1558
		By Dr. HUTTON	-	-	4.446
		750 yards down. zh. 5m.			
		inches.			
Bar. below	-	-	29.920. Ther. 59°.		
Do. above	-	-	29.364. Do. 57°.	yds.	
		Measured height	-	-	176.
		By Dr. MASKELYNE	-	-	171.333
				error —	4.667
		By Dr. HUTTON	-	-	4.888
		800 yards down. zh. 35m.			
		inches.			
Bar. below	-	-	29.930. Ther. 59°.		
Do. above	-	-	29.548. Do. 58°.	yds.	
		Measured height	-	-	126.
		By Dr. MASKELYNE	-	-	117.82
				error —	8.18
		By Dr. HUTTON	-	-	8.2996
		850 yards down. zh. 5m.			
		inches.			
Bar. below	-	-	29.940. Ther. 58°.		
Do. above	-	-	29.716. Do. 58°.	yds.	
		Measured height	-	-	76.
		By Dr. MASKELYNE	-	-	69.36818
				error —	6.63182
		By Dr. HUTTON	-	-	6.4918
		900 yards down. zh. 48m.			
		inches.			
Bar. below	-	-	29.945. Ther. 58°.		
Do. above	-	-	29.850. Do. 58°.	yds.	
		Measured height	-	-	26.
		By Dr. MASKELYNE	-	-	29.351
				error +	3.351
		By Dr. HUTTON	-	-	3.313

From the near agreement of the measured and barometrical heights on the summit of Skiddaw, I had formed sanguine hopes that the barometer would prove a most exact determinator of altitudes, and almost supersede the necessity of having recourse to any other mode; but the subsequent observations lead me to fear that the state of the atmosphere has an effect which we cannot yet account for, and to which we cannot apply a correction. When the operation was discontinued on the evening of the 5th, the air was dry and clear; but on the morning of the 6th, the top of the mountain was clouded about 300 yards down, which might be the reason of the two observations differing by more than four yards.

But the object of this paper is not to surmise, or submit any hypothesis; but faithfully to state the result of operations carried on with all the exactness I was capable of. If what I have done be deemed worthy of any attention by the learned and distinguished Society of which you, Sir, are a member, I shall be most highly gratified, and amply compensated for the toil attending operations on high mountains.

THOMAS GREATOREX.

P. S. Dr. MASKELYNE's rules for determining the height of mountains by the barometer, are these :

1st. Take the difference of the tabular logarithms of the observed barometrical heights at the two stations, considering the four first figures (exclusive of the index) as whole numbers, and the remaining figures to the right as decimals.

2dly. Observe the difference of FAHRENHEIT's thermometer

at the two stations ; multiply this difference by $\frac{454}{1000}$, and add or subtract this product, according as the thermometer was highest at the upper or lower station, which will give an approximate height.

3dly. Take the mean of the two altitudes of the thermometer, and find the difference between this mean and 32° . Multiply the approximate height by this difference, and the product by the decimal fraction .00244. This last correction being added to, or subtracted from the approximate height, according as the mean of the two altitudes of FAHRENHEIT'S thermometer was greater or less than 32° , will give the true height of the upper station in English fathoms.

Dr. HUTTON'S rules.

1st. Let the heights of the barometer at the top and bottom of any elevation intended to be measured, be observed as near the same time as may be, as also the temperatures of the attached thermometers, and also the temperature of the air in the shade at both stations, by means of detached thermometers.

2dly. Reduce these altitudes of the barometer to the same temperature by augmenting the height of the mercury in the colder temperature, or diminishing that in the warmer by its $\frac{1}{9600}$ part for every degree of difference of the two.

3dly. Take the difference of the common logarithms of the two heights of the barometer (so corrected), considering the four first figures (exclusive of the index) as whole numbers, and the rest to the right as decimals, which will give an approximate height.

4thly. Take the mean of the two detached thermometers ; and for every degree which this differs from 31° , take so many times the $\frac{1}{435}$ part of the approximate height ; and *add* them if the mean temperature be *above* 31° ; but *subtract* them if it be *below* 31° ; and the sum or difference will be the true altitude in fathoms.

XX. *On the different methods of constructing a catalogue of fixed stars.* By J. POND, Esq. F. R. S. Astronomer Royal.

Read May 21, 1818.

IN the present state of practical astronomy, the principal object in a national observatory, such as that at Greenwich, is to define the position in the heavens, of the fixed stars and other celestial bodies, at the moment of their passage over the meridian; and we judge of the perfection of the instruments, and of the skill with which they are employed, by the degree of precision with which this operation is performed.

The very great changes and improvements that have taken place within these few years in the instruments of this establishment, are well known to all persons who have interested themselves in its concerns. The liberality of his Majesty's Government has been literally unbounded; and instruments of unusual magnitude and of most difficult construction, have been executed with a success that has exceeded the most sanguine expectation. Such powerful means entrusted to my care, could not but produce, on my part, a continued anxiety that they should be employed in the most advantageous manner. Being constantly under the necessity of reflecting a great deal on the various possible modes, both of making observations and of deducing results from them, I have insensibly been led to adopt methods which differ very materially from those generally pursued,

and some of these appear to me to possess advantages so decided, that I venture to submit them to the consideration of this Society; at the same time, conscious that the subject cannot excite a very general interest, I shall endeavour to abstain as much as possible from every unnecessary detail, and confine myself to the explanation of the general principles of the method which I propose to recommend.

According to the method hitherto invariably followed in this Observatory in constructing a catalogue of stars, either in declination or right ascension, some one star has been taken as a point of departure, and the positions of all the rest determined by direct comparison with this alone. The declinations were determined by direct measurement with γ Draconis;* and α Aquilæ was chosen as the common term of comparison in right ascension. This mode of proceeding with the mural quadrant, though evidently capable of improvement, was not so very objectionable, as when applied to the observations made with the transit instrument; as the observer must be supposed desirous of obtaining the greatest possible accuracy from a given number of observations.

Indeed, in the latter case, the principle is so very objectionable, that I cannot now help expressing some surprize that it should have been employed for so great a length of time. In the first place, every result deduced from the observation of each star is affected with a double error; that committed in the observation of α Aquilæ, and that in the observation

* This will evidently appear to have been the case, if it be considered that the plumb line in the mural quadrant performed no other office than that of maintaining the instrument in a given position.

of the star itself; this objection however is but trifling, compared with a more important one, which is this: if the observation of α Aquilæ be omitted, either from bad weather, or from its passing the meridian at an inconvenient hour, or from neglect, then the observations of all the other stars are, for the purposes of this investigation, rendered entirely useless. Hence arose the necessity of combining the observations of so many years, to construct a catalogue with the accuracy required. That extreme accuracy was ultimately obtained, I am most happy to have it in my power to affirm, since the difference is almost insensible between the catalogue lately deduced from the new transit instrument and that of my predecessor; though an interval of ten years has intervened between the periods of their construction. The advantage therefore of my method I conceive to consist in this; that a catalogue may be constructed from the observations of a single year, equal in accuracy to one which formerly was obtained in three.

The method I propose is equally applicable to the mural circle and the transit instrument. With neither do I assume any particular star as a point of departure, in preference to the rest; but, on the contrary, every star in its turn is assumed as a point of reference to the others; thus endeavouring, in the first instance, to establish their relative distances from each other on the equator, or meridian, leaving the choice and determination of some common point of departure as a subject for future consideration.

To render this more easily intelligible, perhaps it would be better to consider each instrument separately.

Whoever attentively examines the construction of the mural circle at Greenwich, will perceive that its operation is entirely limited to the measurement of the meridional distance between different stars. It is, in fact, a theodolite placed vertically. By an extension of its principle, it can measure the distance between a star and its reflected image from a mercurial horizon, and thus determine the altitude of a star; and, in common with every other circle, it can measure the distance of a circumpolar star from the pole by an observation of its inferior and superior passage over the meridian. As the instrument cannot be reversed, a plumb line could not be applied, with any advantage. In circles that turn freely in azimuth, a double observation of a star gives the angle which the star makes with the axis of the instrument round which it turns, and a plumb line is properly applied in this case to ensure the verticality of this axis; but on the Greenwich circle, a plumb line could only serve as a point of departure; and this having no reference to the real zenith, would, in my opinion, be very injudiciously chosen.

Let us suppose, by way of example, that twelve stars had been observed with the mural circle, and one of these the pole star; moreover, that this star had been observed below the pole. Then, as it would be easy to determine the polar point, the position of all the remaining stars, with reference to this point, might likewise be ascertained. It is however evident in this case, that each result would be affected by a double error, viz. the error committed in the determination of the polar point, and also that due to the observation of each star.

Let us next suppose that, from some accidental cause, the inferior passage of the pole star had been omitted; then, if the declination of this star be supposed to be known from previous observation, the polar point may be deduced from this supposition; but the pole star, in this case, would have no superiority over any other star, whose polar distance should previously have been determined. If we employ two stars equally well known, it is evident that greater accuracy will be obtained; and it is easy to conceive that by taking a greater number of stars, for instance the whole twelve, we may obtain greater accuracy than even by a double observation of the pole star, because the error of observation, and that arising from false assumption of polar distance, will necessarily be much diminished by the natural tendency which the positive and negative errors will have to counteract each other.

These considerations, aided by daily trial and experience, induced me very soon after the mural circle was erected, to abandon every method which assumed one particular point of departure, and I directed all my efforts to the determination of the difference of declination which existed between every star and all the rest; constructing by this means a catalogue, which, with respect to the pole or the zenith, might be subject to a common error; reserving to myself the power of investigating this common error by a process which will afterwards be explained. In the practical execution of this method, the observation of each star is employed for a double purpose; it first serves, in combination with all the others, to find the common index error; this common error again applied to the individual observation, gives a

new result of the position of the star: and of the totality of these results is the catalogue ultimately formed.

In a paper which I had the honour of communicating to the Society, in the year 1816, I have given several examples of the manner in which the index error of the mural circle is calculated, though I did not at that time explain the principle or advantages of this method. In the printed Greenwich observations will be also found the computations of this index error for every month.

I should observe, that when the greatest possible accuracy is required, the whole of the observations should be re-computed from the catalogue now supposed to be in its most perfect state, except the extremely small improvement it may receive from this process. And thus, by continued observation and approximation, may the *relative* places of the stars be assigned to a very unexpected degree of accuracy. Now, though a continuation of this process has an evident tendency to improve the catalogue, as far as concerns the relative distances of the several stars to each other, yet it can have no effect in correcting any common error that may exist from a false assumption either of the zenith or the pole. I will endeavour, therefore, to explain the next part of the process, which is to determine the common error in polar distance. It is to be presumed that the pole star forms one of the stars of which the intended catalogue is composed, and it should be observed most assiduously, both above and below the pole. These observations are to be treated as if appertaining to different stars, and the place of each determined in the catalogue, by applying the same index error as that em-

ployed for the other stars. When the whole catalogue is completed, these two results are to be examined; and, if they appear equally distant from the pole, the catalogue is affected by no common error; but if, on the contrary, the polar point is not found to be precisely between these results, then half the difference will be the common error.

The details of these computations will be found in the Greenwich observations; and it will there be seen that the polar distances of this star, determined by 158 observations, in the same manner as any other star of the catalogue, namely, by the application of the common index error, was found to be

Below the pole, by 132 observations, computed

in the same manner = 1.41.21.50

Their sum - - 358.18.38.32

360

Difference from 360 - 0 0 0".18

Difference or common error = 0.09

This difference, 0".09, is the common error to be added to each star of the catalogue, and the polar distance of the star thus corrected will be 1.41.21.59.

I find by about 350 observations, deduced in the usual manner, without any reference to the other stars, 1.41.21.65, reduced to the beginning of 1813.

The same principle, which I have thus attempted to explain, may be applied with equal facility, and even greater advantage, to the formation of a catalogue in right ascension. The

error of the clock in this case, answers to the index error of the circle, and is investigated in precisely the same manner. An approximate catalogue is first assumed; and here, as with the circle, the observation of each star serves a double purpose: in the first place, in common with all the rest, it is employed to determine the error of the clock; and the error thus found is applied to the observation of the star. In this case, the right ascension of the star is supposed to be known; in the second part of the process, the star is considered as a planet, or unknown object, and its right ascension found by the usual rules, and recorded as a single result; and from the totality of these results is the right ascension ultimately obtained.

Should the deduced right ascensions differ materially from those originally assumed, the improved catalogue must be substituted for the approximate one, and the whole process recomputed. In this substitution, however, some judgment and discretionary power must be exercised; for should the assumed or approximate catalogue be very exact, and the subsequent observations few or inaccurate, it is evident that the new catalogue might be less correct than the assumed one; this, however, does not arise from any defect in the method, but is the inevitable consequence of any attempt to improve good observations by bad ones. As a practical illustration of this remark, I might add, that having myself assumed an approximate catalogue, so exact as that of Dr. MASKELYNE, it happens that at this moment, in the case of ~~some~~ stars which have not been very frequently observed, I have great doubts whether the new determination is more

exact than the old one; but by continuing this process, a proper ascendancy will necessarily be acquired by the latest observations.

In comparing my catalogue of right ascensions with that of Dr. MASKELYNE, I meet with a singular coincidence, which seems to me to illustrate and confirm, in a very striking manner, the advantage of the principle in question. In each catalogue, the right ascension of α Aquilæ, though deduced apparently by a different process, comes out the same, even to the hundredth part of a second. Accident may possibly have some little share in this very exact coincidence, but it appears to me chiefly to arise from the very nature of our respective processes. In Dr. MASKELYNE's method, the right ascension of every star is derived from direct comparison with α Aquilæ, or in other words, the right ascension of α Aquilæ is derived by comparing it with every star. So it is in my method; and hence the same result ought to be obtained. But the advantage which in one case is peculiar to α Aquilæ, is in the other method extended to every star: no possible reason can be given, why the place of one star should be more accurately assigned than that of another, provided an equal number of observations be obtained of each, since the place of every star is deduced exactly in the same manner from a comparison of all the rest.*

Though not immediately connected with the present subject, I wish to take this opportunity of stating, that, in comparing the observations of the old transit instrument with those of the new one, I find a much less difference than I

* As I have nothing new to offer, as to the method of deducing the equinoctial point from observations of the sun, I have not taken this part of the subject into consideration.

expected. It appears to me that the former instrument must have described nearly a correct hour circle, though this circle was evidently not exactly the meridian. With all the decided superiority of the new instrument, I cannot venture to assign to the catalogue of my predecessor an error much greater than one tenth of a second of time, even in the stars near the horizon, where the error appears probably to have been the greatest. I trust it will be considered by astronomers, as creditable to the history of this Observatory, that two observers, with different instruments (and by as different a method of computation as the case admits of) should deduce two catalogues so exactly alike, that they may be considered almost as identical.

It is an interesting question to every astronomer possessed of a valuable instrument, to know to what degree of accuracy its results can be depended on. I have examined a great number of observations made with the new transit instrument with this view, and it appears to me, that near the equator, 60 observations will generally give the second decimal place of a second of time very correctly; 120 observations will give this with greater certainty, but not in the proportion of two to one.* This I think is rather a greater exactness than can be obtained by the mural circle,† and the reason I apprehend to be this.

* I find, by the rule given by M. LAPLACE, that the probable error of 120 observations is $0^{\circ}.005$ in time.

† The optical power of the new transit instrument is so decidedly greater than in the former one, that each observation must necessarily be more exact; but I do not find the discordances in the ultimate results smaller in the same proportion: from this circumstance I conclude, that the limit to accuracy consists rather in the clock, than in the instrument.

The Greenwich transit clock, compared with others, is, I believe, considered as a

With the mural circle, or any similar instrument, the place of a star appears to be defined at the moment of observation with much greater precision than by a transit instrument ; but the error of the latter, I conceive, to be much more purely accidental ; so that the result of a great number of observations will be more exact. Local refraction places an insurmountable limit to observations in declination, particularly at any sensible distance from the zenith : observations in right ascension are free from this material source of error, and are therefore susceptible of attaining greater accuracy by continued perseverance.

I have no doubt but to many persons, and even to those extremely conversant with the theory of astronomy, it may appear, that a very undue degree of importance is attached by practical astronomers to the investigation of such minute quantities as form the subject of this paper ; and it is a question often asked, of what importance can it be to science, that the place of a fixed star should be so accurately ascertained ? A person in my situation might reply, that being employed to do this, it is incumbent on him to do it in the best manner possible, leaving the question of utility to others ; but there is really a very reasonable and satisfactory answer to be given to this question. In the first place, since the position of the sun, moon, and planets, are deduced from direct comparison with the principal stars, it is requisite, for this purpose, that their places should be accurately known, and their various changes ascertained. Moreover,

very good one ; but of all the instruments in the Observatory, it is certainly that in which improvement would be most beneficial.

though in common language we speak of determining the places of the fixed stars relatively to the equator and ecliptic, the real fact is, that the stars being stationary, it is the situation of these circles themselves, and the point of their intersection, that is the ultimate object of research ; and in this point of view we at once perceive how these precise determinations may facilitate the most abstruse investigations of physical astronomy.

XXI. *A description of the teeth of the Delphinus Gangeticus.*
By Sir Everard Home, Bart. V. P. R. S.

Read June 4, 1818.

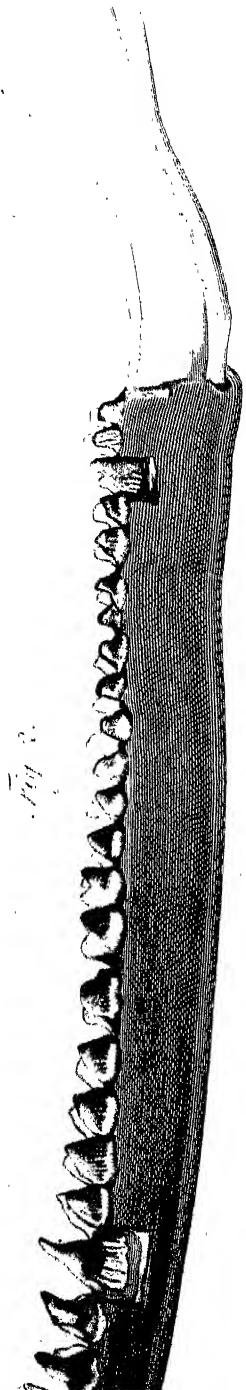
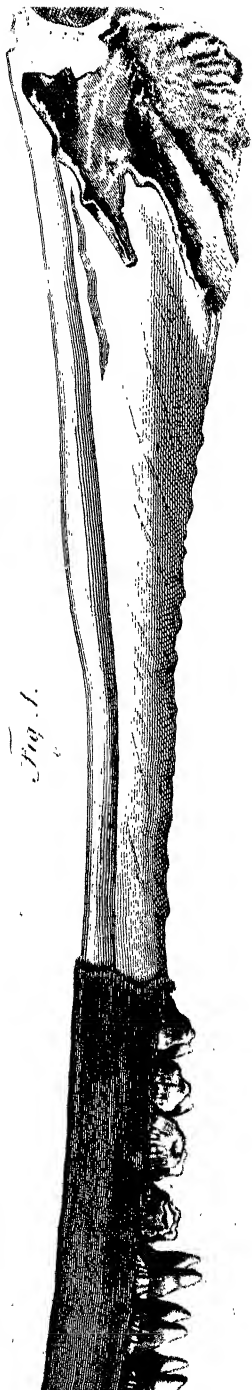
THE *Delphinus Gangeticus* is described by the late Dr. ROXBURGH, in the seventh volume of the *Asiatick Researches*, published in 1781; but no farther account is given of its teeth, than that the number is 120, 30 in each jaw, nor have I met with any description of them in other publications. Dr. SHAW published his second volume of *General Zoology*, in which the whale tribe is mentioned, in 1801. He says, a narrow snouted dolphin is supposed to inhabit the Indian seas; but it is only known to us from specimens of the head and jaws. The jaws are extremely narrow, the teeth small, not numerous, distant, and shaped somewhat like the molares of quadrupeds. This description corresponds so ill with the teeth of the *Delphinus Gangeticus*, that it would almost induce us to believe that it is meant for those of another animal.

A specimen of the upper and lower jaw of the *Delphinus Gangeticus* was given to me, 17 years ago, by Sir JOSEPH BANKS, and has been deposited ever since in the HUNTERIAN Collection; but it was only the other day that an accidental reference to the *Asiatick Researches* led us to discover the animal to which they belong. The singularity of the teeth made it always a remarkable object, and now the animal is known, a description of them may not be undeserving the attention of this Society.

The jaws and teeth form the most remarkable characters of this species of delphinus, and a description of them will not only interest the naturalist and comparative anatomist, but will enable the geologist, when fossil teeth are found of this shape, readily to determine the species of animal to which they had belonged.

These teeth, as is common in those of the whale tribe, have the first rudiments formed in the gum, from which the tooth grows in both directions, upwards through the gum in the form of the point of a flattened cone which is coated with enamel, and downwards towards the jaw, increasing considerably in breadth but not in thickness, till it is at last embedded in the substance of the jaw itself; the lower portion has no enamel.

The appearance the teeth put on, as well as the mode of growth, will be seen in the annexed drawing; [Pl. xx.] the change that takes place in the form of the tooth as it wears away from long use, is more remarkable than in most other teeth; for the perfect tooth has a tolerably sharp enamelled point, while the half worn one has a curved blunted cutting edge. The teeth in front of the jaw are more like the incisores in other animals. The whole number in both jaws is 120, which exactly corresponds with that given by Dr. ROXBURGH, and identifies these jaws as belonging to the animal which he has described.



EXPLANATION OF PLATE XX.

Fig. 1. A side view of the upper jaw, to show the form of the teeth.

Fig. 2. The same view of the under jaw. Both these figures are on a scale of $\frac{1}{4}$ inches to a foot.

Fig. 3. One of the most perfect of the teeth, of its natural size ; it is taken from the lower jaw.

Fig. 4. A tooth in its growing state, represented of the natural size, from the lower jaw.

XXII. *Description of an acid principle prepared from the lithic or uric acid.* By William Prout, M. D. Communicated by W. H. Wollaston, M. D. F. R. S.

Read June 11, 1818.

DURING an investigation of the principles of the urine, with the view of elucidating the pathology of that secretion, I was led to examine the well-known beautiful purple substance produced by the action of the nitric acid and heat upon the lithic acid, and which has usually been considered as one of the characteristic distinctions of the lithic acid. This purple substance proved to be a compound of ammonia, and a peculiar principle having the properties of an acid; the description of which, and of its compounds, constitutes the object of the present paper.

This acid principle may be obtained by digesting pure lithic acid in dilute nitric acid: an effervescence takes place, and the lithic acid is dissolved. The excess of nitric acid is then to be neutralized with ammonia, and the whole slowly concentrated by evaporation. As the evaporation proceeds, the colour of the solution gradually becomes of a deeper purple, and dark red granular crystals (sometimes of a greenish hue externally) soon begin to separate in abundance. These crystals are a compound of ammonia with the acid principle in question. The ammonia may be removed by the sulphuric or muriatic acid, and thus the acid principle obtained in a separate state. As, however, I found some

little care requisite to obtain the acid quite free from colour, it may not be deemed superfluous to state the precise method I usually followed for that purpose. The compound with ammonia, above-mentioned, was dissolved in a solution of caustic potash, and heat applied to the solution till the red colour entirely disappeared. This alkaline solution was then gradually dropped into dilute sulphuric acid, which uniting with the potash, left the acid principle in a state of purity.

The acid principle is likewise produced from lithic acid by chlorine. Iodine has also the same remarkable property, though in a much less striking degree. When lithic acid is boiled with iodine for some time, a partial solution of the lithic acid is effected; and if to this solution a little ammonia be added, and the whole evaporated to dryness, a perceptible quantity of the beautiful purple compound of ammonia and the new acid principle will be obtained. I am not aware that any other substance is capable of producing this change, though the circumstance is by no means improbable.

To prevent circumlocution, I shall in future call this principle the *purpuric acid*, a name suggested by Dr. WOLLASTON, from its remarkable property of forming compounds with most bases of a red or purple colour.

The purpuric acid, as obtained above, usually exists in the form of a very fine powder, of a slightly yellowish or cream colour; and when examined with a magnifier, especially in water, appears to possess a pearly lustre. It has no smell nor taste. Its specific gravity is considerably above that of water, though from the minute state of division in which it exists, it usually takes a considerable time to subside in that fluid. When suffered to separate slowly from a large quantity

of water, or any other fluid capable of holding it in solution, it sometimes assumes the form of thin pearly scales.

The purpuric acid is very little soluble in water. One tenth of a grain boiled for a considerable time in 1000 grains of water was not entirely dissolved. The water assumed a purple tint, which it retained after it was cold, though it became very slightly turbid on cooling.* The purpuric acid is insoluble in alcohol and ether. In all the mineral acids, when concentrated, and in excess, and in solutions of the different alkalis, it dissolves readily; but it is insoluble, or nearly so, in dilute sulphuric, muriatic, and phosphoric acids, and also in solutions of the oxalic, citric, and tartaric acids. Concentrated nitric acid readily dissolves it with effervescence; and if the acid be in excess, and heat be applied, a portion of the purpuric acid is decomposed, ammonia is formed, and on driving off the excess of nitric acid by heat, the purpurate of ammonia is obtained, precisely as if a little of the lithic acid had been treated in a similar manner. Chlorine, likewise, dissolves the purpuric acid, and apparently produces the same changes upon it as the nitric acid. It readily dissolves also by the assistance of heat, in concentrated acetic acid.

The purpuric acid does not sensibly affect litmus paper, probably on account of its insoluble nature. When exposed to the air it does not deliquesce, but gradually assumes a purplish tint, apparently by attracting a little ammonia from

* I am not quite sure whether the purple tint here mentioned depends upon the actual solution of a minute portion of the purpuric acid; and, consequently, whether it naturally forms a purple solution, or whether the colour be owing to the formation of a little ammonia from the decomposition of a minute proportion of the acid, which, combining with the remainder of the acid, forms the purpurate of ammonia. I incline to the latter opinion.

the atmosphere, or perhaps from the evolution from itself of a little of the same alkali by spontaneous decomposition.

Submitted to heat, it neither melts nor sublimes, but acquires a purple hue from the formation of ammonia, and afterwards burns gradually, without yielding any remarkable odour. Subjected alone to heat in close vessels, it yields a considerable proportion of the carbonate of ammonia, some prussic acid, and a little fluid having an oily appearance; while a portion of pulverulent charcoal remains. When given quantities were burnt with the oxide of copper, in the manner formerly described by me,* data were obtained, which appeared to show that one hundred parts consist of

Hydrogen	4.54	corresponding with 2 atoms or proportions.
Carbon	27.27	_____ 2 ditto.
Oxygen	36.36	_____ 2 ditto.
Azote	31.81	_____ 1 ditto.

The purpuric acid combines with the alkalies, alkaline earths, and metallic oxides. It is capable of expelling the carbonic acid from the alkaline carbonates, by the assistance of heat, and does not, as far as I have observed, combine with any other acid. These are circumstances sufficient, as Dr. WOLLASTON has observed, to distinguish it from an *oxide*, and to establish its character as an *acid*. On the supposition then, that it be named the *purpuric acid*, its compounds with different bases must be denominated *purpurates*: on some of the most remarkable of which I shall now proceed to make a few remarks.

Purpurate of ammonia. This salt crystallizes in quadrangular prisms, which, when viewed by transmitted light, are

* See Medico-Chirurgical Transactions, vol. viii. p. 526.

transparent, and of a deep garnet red colour; but by reflected light, their two broadest opposite faces appear of a brilliant green, closely resembling that of the wings of some of the beetle tribe, as for example, of the *Cetonia aurata*, while their other two opposite faces appear of a dull reddish brown colour; or if the light be very strong, slightly green. This peculiarity seems to be possessed in a greater or less degree by all the other alkaline, and perhaps earthy salts; and doubtless depends upon the structure of the crystals. The purpurate of ammonia is soluble in about 1500 parts of water at 60° , but in boiling water is much more soluble. The solution is of a beautiful deep carmine, or rose red colour. In pure alcohol and in ether, it is little if at all soluble. The aqueous solution has a slightly sweetish taste, but no smell. By adding this aqueous solution of the purpurate of ammonia to neutral saline solutions of other bases, most of the following purpurates were formed.

Purpurate of potash. When a saturated boiling solution of the purpurate of ammonia is added to a solution of the bicarbonate of potash, a dark brownish red precipitate takes place, which is the purpurate of potash. If, however, this salt be slowly formed, it may be obtained in a crystalline form; and the crystals appear to possess the same peculiarity with respect to colour, as those of the purpurate of ammonia above-mentioned. This salt is much more soluble than the purpurate of ammonia.

Purpurate of soda. This salt, when obtained by the same means as the purpurate of potash, is of a dark brick red colour. It may, however, be obtained in crystals. It is much less soluble than the purpurate of potash. Three thousand times its weight of water at 60° , did not completely dissolve

it. The colours of the solutions of this salt, and of potash, differ slightly from one another, and also from that of the purpurate of ammonia; but it is not easy to describe these differences so as to render them intelligible.

Purpurate of lime. This salt, when obtained by adding a boiling saturated solution of the purpurate of ammonia to a solution of the muriate of lime, exists in the form of a powder much resembling in colour the crust of the lobster before it is boiled. This salt is but little soluble in cold water; but in boiling water it is more soluble, and the solution is of a beautiful reddish purple colour.

Purpurate of strontian. This salt obtained as above, with the nitrate of strontian, exists in the state of a dark brownish red powder, with a slight tinge of green. It seems to be more soluble than the purpurate of lime, and forms a purple solution.

Purpurate of barytes. Obtained as before described, with the acetate of barytes, this salt assumes the form of a dark green powder, not apparently differing much in point of solubility from the purpurate of strontian; and forming, like that salt, a purple solution.

Purpurate of magnesia. This is a very soluble salt. Its solution is of a beautiful purple.

Purpurate of alumina. When a solution of the purpurate of ammonia was added to a solution of alum, no perceptible change took place immediately; but after some time the colour of the solution disappeared, and a small quantity of a white substance separated, which was presumed to be the purpurate of alumina, but it was not examined.

Purpurate of gold. When a solution of the muriate of gold is dropped into a solution of the purpurate of ammonia, the

colour becomes yellowish, but no precipitation takes place. Hence, this salt may be presumed to be very soluble.

Purpurate of platina. The muriate of platina changes the colour of the purpurate of ammonia to a yellowish scarlet, but produces no precipitation.

Purpurate of silver. Solutions of the acetate or nitrate of silver, dropped into a solution of the purpurate of ammonia, produce a deep purple precipitate; and the water is left nearly colourless. Hence the purpurate of silver appears very insoluble.

Purpurate of mercury. A solution of the proto-nitrate of mercury produces, with the purpurate of ammonia, a beautiful reddish purple precipitate, and the water is left nearly colourless. A solution of the oxymuriate of mercury produces at first no change; but after some time a copious light rose coloured precipitate occurs, and the solution is left colourless.

Purpurate of lead. A solution of the nitrate of lead, dropped in a solution of the purpurate of ammonia, renders it of a rose red colour; but no precipitation takes place.

Purpurate of zinc. A solution of the acetate of zinc produces with the purpurate of ammonia, a solution and precipitate of a beautiful gold yellow colour; and a most brilliant iridescent pellicle, in which green and yellow predominate, forms on the surface of the solution.

Purpurate of tin. A solution of the muriate of tin changes the purpurate of ammonia to a scarlet; but this rapidly disappears, and the solution becomes colourless. After a few hours, white pearly crystals form in abundance, which is the purpurate of tin.

Purpurate of copper. A solution of the acetate or sulphate of copper changes the purpurate of ammonia to a bright yellowish green colour, but produces no precipitation.

Purpurate of nickel. The nitrate of nickel imparts to the purpurate of ammonia a greenish tinge, but produces no precipitation.

Purpurate of cobalt. The acetate of cobalt changes the colour of the same salt to a pale scarlet. After some time, reddish granular crystals form, which are the purpurate of cobalt.

Purpurate of iron. A solution of the green sulphate of iron changes the colour of the purpurate of ammonia to yellowish red, but produces no precipitate.

Such is a very brief account of the *purpurates*, as far as I have examined them. It may at first sight appear singular, that such an insoluble acid should form so many soluble compounds; but when we reflect upon the subject, and consider what a very small quantity of the purpurate of ammonia is retained in solution by water, and that this small quantity has been made the standard of comparison in the above experiments, our surprize is considerably lessened, and we feel no difficulty in conceiving, that if the purpurates were compared with the nitrates, for example, the former would be found by far the least soluble.

From the very small quantities on which I have been obliged to operate, and from other circumstances, I can offer but little respecting the constitution of the purpurates. Those which I have attempted to analyze appear to be anhydrous, and to be composed of two atoms of the acid, and one of the base; and if this be correct, the

same composition may perhaps be referred to most, if not all the compounds above mentioned. The purpuric acid, however, appears capable of forming subsalts and supersalts, with most bases, many of which seem to be very little soluble.

With respect to the characteristic properties of the purpuric acid, I apprehend it may be readily distinguished from all other substances by the beautiful colours exhibited by its alkaline and earthy salts, independently of its other properties, which are likewise peculiar.

The purpuric acid, and its compounds, probably constitute the basis of many animal and vegetable colours. The well known pink sediment, which generally appears in the urine of those labouring under febrile affections, appears to owe its colour chiefly to the purpurate of ammonia, and perhaps occasionally to the purpurate of soda. Some of the purpurations, as for example that of lime, might be probably used as a paint. They might be also used for dyeing, especially wool and other animal productions.* On this part of the subject, however, as I have little that is certain to offer, I do not deem it prudent to enter at present.

* I may here observe, that the solution of lithic acid in nitric acid, has the property of tinging the skin and other animal substances in a very permanent manner. The colour does not, in general, appear till the substance has been exposed to heat, or what is more effective, to the light of the sun. In the latter case, particularly, a deep purple tint soon makes its appearance, and the substance tinged (more especially the skin) emits during the process a strong and peculiar smell, closely resembling that produced by the nitrate of silver, when applied to the skin, and exposed to similar circumstances.

XXIII. *Astronomical observations and experiments, selected for the purpose of ascertaining the relative distances of clusters of stars, and of investigating how far the power of our telescopes may be expected to reach into space, when directed to ambiguous celestial objects.* By Sir William Herschel, Knt. Guelph. LL.D. F. R. S.

Read June 11, 1818.

IN my last paper on the local arrangement of the celestial bodies in space, I have shown how, by an equalization of the light of stars of different brightness, we may ascertain their relative distances from the observer, in the direction of the line in which they are seen; and from this equalization, a method of turning the space-penetrating power of a telescope into a gradually increasing series of gaging powers has been deduced, by which means the profundity in space, of every object consisting of stars, can be ascertained, as far as the light of the instrument which is used upon this occasion will reach. This method has already been applied to fathom the milky way, and may with equal propriety be used to ascertain the profundity of globular and other clusters of stars in space; I shall therefore make use of some of the numerous observations, contained in my journals and sweeps of the heavens, to show how the distances of these objects may be obtained; and shall also attempt to represent their situation in space by a figure, in which their distances are made proportional to the diameter of a globular space, sufficiently large

to contain all the stars that in the clearest nights are visible to the eye of an observer.

I. Of the distance of globular and other clusters of stars.

In observations which are made for ascertaining the distance of a cluster of stars, it is necessary that the gaging power should be marked, which will just make some of the stars belonging to it visible in the telescope that is used for this purpose. If the cluster is of a globular form, but is not insulated, the stars that belong to it may be easily distinguished from those which may happen to be scattered about, or upon it. In clusters of a different construction, the compression, or the apparent size of the stars, must direct the observer.

It is to be remarked, that neither the brightness, nor the diameter of the clusters of which the distance is to be ascertained, are to be considered: some of them are bright enough to be perceived by the eye; others are visible in the finder of the telescope, and many of them can only be seen in the telescope itself. These are circumstances that have no influence on the exactness of the result of the gaging power; but as they regard our knowledge of the construction of these magnificent sidereal systems, an abridged account of them is given, with the observations by which their profundity in space is ascertained; and in the arrangement of these observations, I have followed the order of the space-penetrating power of the instruments by which they were made.

In recording the examination of celestial objects, I have often applied to them the expressions *resolvable*, or *easily resolvable*, when, from their appearance, I could not decide

whether they belonged to the class of nebulae, properly so called, or whether they might not consist of an aggregation of stars, at too great a distance from us to be distinctly perceived; but it is evident that the distance of a cluster of stars cannot be ascertained, as long as it remains doubtful whether the object consists of stars; and that, consequently, their first perceptibility must be the gaging power by which its profundity in space is to be ascertained.

II. *A series of observations of clusters of stars, from which the order of their profundity in space is determined.*

*Observation of the 7th cluster of stars in the vith class of my catalogues of celestial objects.**

“ 1784, 20 feet telescope, power 157. An excessively faint cluster of stars, intermixed with resolvable nebulosity, 8 or 10 minutes in diameter. The stars are so small that they cannot be seen without the greatest attention; 240 verified it beyond all doubt. I have suspected many such in this neighbourhood.”

At the time of this observation, the 20 feet telescope was of the NEWTONIAN construction, and its power to penetrate into space was 61.18 times that of the eye, which it has been shown can see stars of the 12th order :† and since it appears from the foregoing observation, that with this power the telescope could but just reach the stars of the cluster, we may conclude that its profundity in space cannot be less than of the 734th order.

* For these catalogues, see Phil. Trans. for 1786, page 471; 1789, page 226, and 1802, p. 503.

† Phil. Trans. for 1817, page 317.

Observation of the 9th cluster in the vith class.

“ 1784, 20 feet telescope. A large cluster of exceedingly
 “ small, and compressed stars, about 6 or 7 minutes in
 “ diameter; a great many of the stars are visible, the rest so
 “ small as to appear nebulous; those that are visible are of
 “ one size, and are scattered all over equally. The cluster is
 “ of an irregular round form.”

The profundity of the cluster by this observation is of the 734th order.

Observation of the 10th cluster in the vith class.

“ 1784, 20 feet telescope. A very compressed, con-
 “ siderably large cluster of the smallest stars imaginable;
 “ all the stars are of a dusky red colour. This cluster is the
 “ next step to an easily resolvable nebula.”

The ruddy colour of the stars is probably owing to its low situation; the profundity of the cluster is of the 734th order.

Observation of the 11th cluster in the vith class.

“ 1784, 20 feet telescope. A cluster of stars, which, in
 “ respect of the size of the whole as well as the distance and
 “ magnitude of the stars, is a good miniature of the 19th of
 “ the connoissance observed a few minutes ago. The stars,
 “ like those of the foregoing cluster, preserve a faint red
 “ tint. It may be called the next step to an easily resolvable
 “ nebula. It is about $1\frac{1}{2}$ or 2 minutes in diameter.”

The profundity of this cluster cannot be much less than of the 734th order. It is in the preceding branch of the milky way.

Observation of the 12th cluster in the vith class.

“ 1784, 20 feet telescope. This cluster of stars is another
“ miniature of the 19th of the connoissance, but rather coarser
“ than my 11th cluster.”

The profundity of the 19th of the connoissance being of the 344th order, this cluster, as rather a coarse miniature of it, may be of the 466th order; it is in the preceding branch of the milky way.

Observations of the 17th cluster in the vith class.

“ 1784, 1785, 20 feet telescope. A very rich cluster of
“ very compressed and extremely small stars; 4 or 5
“ minutes in diameter.”

This cluster is probably of a profundity of about the 600th order. It is in the preceding branch of the milky way.

Observations of the 20th cluster in the vith class.

“ 1785, 1786, 20 feet telescope. Considerably bright,
“ irregularly round, 8 or 9 minutes in diameter; a great
“ many of the stars are visible, so that there can remain no
“ doubt of its being a cluster of the most minute stars
“ imaginable.”

The profundity of this cluster cannot be less than of the 734th order. It is near the south pole of the milky way.

Observation of the 26th cluster in the vith class.

“ 1786, 20 feet telescope. A very faint cluster of very
“ compressed extremely small stars; near 4 minutes in
“ diameter.”

The 20 feet telescope being of the construction of the

front view, and having a gaging power of 75.08 gives the profundity of this cluster of the 900th order. It is in the milky way.

Observation of the 35th cluster in the vith class.

“ 1788, 20 feet telescope. A small cluster of very faint, exceedingly compressed stars, about 1 minute in diameter ; the next step to an easily resolvable nebula.”

The profundity of this cluster is of the 900th order ; it is in the milky way.

Observation of the 38th cluster in the vith class.

“ 1791, 20 feet telescope. Considerably bright, small, of an irregular figure ; easily resolvable : some of the stars are visible.”

The profundity of this cluster is of the 900th order. It is in the milky way.

Observation of the 41st cluster in the vith class.

“ 1797, 20 feet telescope. Round, resolvable, about 3 minutes in diameter ; very gradually brighter in the middle. I suppose it to be a cluster of extremely compressed stars ; 320 confirms the supposition, and shows a few of the stars : it must be immensely rich.”

The profundity of this cluster is of the 900th order.

Observation of the 69rd cluster in the xvth class.

“ 1789, 20 feet telescope. Considerably bright, considerably large, irregularly round, very gradually much brighter in the middle ; about 4 minutes in diameter.”

The profundity of this cluster must be at least of the 900th order.

Observations of the 1st of the connoissance des temps.

“ 1783, 1794, 7 feet telescope. With 287, light without stars.”

“ 1805, 1809, 10 feet telescope. It is resolvable. There does not seem any milky nebulosity mixed with what I take to be small lucid points.”

“ 1783, 1784, 1809, 20 feet telescope. Very bright, of an irregular figure ; full 5 minutes in the longest direction. I suspect it to consist of stars.”

“ 1805, large 10 feet telescope. With 220 the diameter is 4' 0"; with this power and light it is what must be called resolvable.”

As all the observations of the large telescopes agree to call this object resolvable, it is probably a cluster of stars at no very great distance beyond their gaging powers ; its profundity may therefore be of about the 980th order. It is near the milky way.

Observations of the 2nd of the connoissance.

“ 1799, 7 feet finder of the telescope. It is visible as a star. 1810, it may just be perceived to have rather a larger diameter than a star.”

“ 1783, 2 feet sweeper. It is like a telescopic comet.”

“ 1794, 7 feet telescope. With 287 I can see that it is a cluster of stars, many of them being visible.”

“ 1810, 10 feet telescope. A beautiful bright object.”

“ 1784, 1785, 1802, 20 feet telescope. A cluster of very compressed exceedingly small stars.”

" 1805, 1810, large 10 feet telescope. Its diameter with
 " 108 is 4' 59"; with 171 and 220, it is 6' 0"."

" 1799, 40 feet telescope. A globular cluster of stars."*

By the observation of the 7 feet telescope, which has a power of seeing stars that exceeds the power of the eye to see them 20.25 times, the profundity of this cluster is of the 243rd order.

Observations of the 3rd of the connoissance.

" 1813, 7 feet finder. It is at a small distance from a star
 " of equal brightness; the star is clear, the object is hazy,
 " and somewhat larger than the star."

" 1783, 7 feet telescope. With 460 the light is so feeble
 " that the object can hardly be seen; I suspect some stars in
 " it. 1813, with 80, many stars are visible in it."

" 1799, 10 feet telescope, power 120; with an aperture of
 " 4 inches it is resolvable; with 5 easily resolvable; with 6
 " it is resolved; with 7 and all open the stars may be easily
 " perceived."

" 1784, 1785, 20 feet telescope. A beautiful globular
 " cluster of stars, about 5 or 6 minutes in diameter."

" 1810, Large 10 feet telescope. With 171 the diameter
 " is full 4' 30".

By the observation of the 7 feet telescope this cluster must be of the 243rd order.

Observations of the 4th of the connoissance.

" 1783, 10 feet telescope. All resolved into stars. I can
 " count a great number of them, while others escape the eye
 " by their faintness."

* For the particulars of this observation see Phil. Trans. for 1814. page 274.

for ascertaining the distances of clusters of stars, &c. 437

“ 1783, small 20 feet telescope. All resolved into stars.”

“ 1784, 20 feet telescope. The cluster contains a ridge of stars in the middle, running from south preceding to north following.”

The 10 feet telescope having a power to show stars exceeding that of the eye 28.67 times, gives the profundity of this cluster of the 344th ord r.

Observations of the 5th of the connoissance.

“ 1813, 7 feet finder. It is near a star of equal brightness; the star is clear but the object is hazy.”

“ 1783, 7 feet telescope. It consists of stars; they are however so small that I can but just perceive some, and suspect others. 1810, the globular figure is visible.”

“ 1783, 10 feet telescope. With 600, all resolved into stars.”

“ 1785, 1786, 20 feet telescope. A very compressed cluster of stars, 7 or 8 minutes in diameter; the greatest compression about 2 or $2\frac{1}{2}$ minutes.”

“ 1791, 40 feet telescope. With 370 the stars about the centre are extremely compressed.”

The profundity of this cluster, by the observation of the 7 feet telescope, is of the 243rd order.

Observations of the 9th of the connoissance.

“ 1783, 10 feet telescope, power 250. I see several stars in it; and have no doubt a higher power and more light will resolve it all into stars.”

" 1784, 1786, 20 feet telescope. A cluster of extremely compressed stars; it is a miniature of the 53d."

By the observations of the 10 feet the profundity is at least of the 344th order. It is in the preceding branch of the milky way.

Observations of the 10th of the connoissance.

" 1783, 7 feet telescope. With 227 I suspected it to consist of stars; with 460 I can see several of them, but they are too small to be counted."

" 1784, 1791, 20 feet telescope. A beautiful cluster of extremely compressed stars; it resembles the 53d; and the most compressed part is about 3 or 4 minutes in diameter."

The profundity of this cluster, by the observation of the 7 feet telescope, is of the 243d order.

Observations of the 11th of the connoissance.

" 1799, 10 feet finder. The cluster is visible; and, directed by neighbouring stars, it may be seen by the eye."

" 1783, 1799, 10 feet telescope. Power 300. With 3 inches of aperture, the small stars are not to be distinguished; with 4 inches I can see them."

" 1803, 1810, large 10 feet telescope. The cluster is of an irregular form, from 9 to 12 minutes in diameter."

The 10 feet telescope with an aperture of 4 inches, had a gaging power of 12.02; the profundity of this cluster is therefore of the 144th order. It is in the milky way.

Observations of the 12th of the connoissance.

" 1799, 10 feet finder. The object is visible in it."

“ 1783, 1799, 10 feet telescope. With 120, and an aperture of 4 inches, easily resolvable; with 5 inches, stars become visible; with 6 inches, pretty distinctly visible; and with all open, the lowest power shows the stars.”

“ 1785, 1786, 20 feet telescope. A brilliant cluster, 7 or 8 minutes in diameter; the most compressed parts about 2 minutes.”

With an aperture of 5 inches the 10 feet telescope had a gaging power of 15.53; and this cluster is consequently of a profundity of the 186th order.

Observations of the 13th of the connoissance.

“ 1799, 1805. It is very plainly to be seen by the eye.”

“ 1799, 7 feet finder. Very visible.”

“ 1783, 7 feet telescope. With 227 plainly resolved into stars.”

“ 1799, 10 feet telescope. With an aperture of 4 inches the stars cannot be distinguished; with 9 inches, very beautiful.”

“ 1787, 1799, 20 feet telescope. The stars belonging to the cluster extend to 8 or 9 minutes in diameter; the most compressed part about 2 or $2\frac{1}{2}$; the latter is round, the former irregular.”

“ 1805, large 10 feet telescope. A brilliant cluster all resolved into stars.”

By the observation of the 7 feet telescope, the profundity of this cluster is nearly of the 243d order.

Observations of the 14th of the connoissance.

1783, 7 feet telescope. With 227, there is a strong suspicion of its consisting of stars.

" 1783, 1784, 1791, 1799, 20 feet telescope. Extremely bright, round, easily resolvable; with 300 I can see the stars. The heavens are pretty rich in stars of a certain size, but they are larger than those in the cluster, and easily to be distinguished from them. The cluster is considerably behind the scattered stars, as some of them are projected upon it."

From the observations of the 20 feet telescope, which in 1791 and 1799 had the power of discerning stars 75.08 times as far as the eye, the profundity of this cluster must be of the 900th order.

Observations of the 15th of the connoissance.

" 1799. It is visible to the eye."

" 1783, 1794, 7 feet telescope. With 278 the stars of the cluster may be seen."

" 1799, 10 feet telescope. With an aperture of 4 inches, no trace of stars is visible. 1817, with an aperture of 4.56 inches, which gives a gaging power of 14, it appears like a nebulous patch, gradually brighter in the middle; with a gaging power of 16, the hazy border of it is larger; with 18, the whole of it much larger and brighter; with 20, resolvable; and with 22, the stars are visible."

" 1784, 1787, 1807, 20 feet telescope. A globular cluster of stars, about 6 minutes in diameter."

" 1810, large 10 feet telescope. The diameter, with 171,

for ascertaining the distances of clusters of stars, &c. 44¹

“ is full 4' 30", and taking in the stars that probably belong
“ to it, it is 6' 45". ”

By the observation of the 7 feet telescope, the profundity of this cluster is of the 243d order.

Observations of the 19th of the connoissance.

“ 1783, 10 feet telescope. With 250, I can see 5 or 6
“ stars, and all the rest appears mottled like other objects of
“ this kind, when not sufficiently magnified or illuminated. ”

“ 1784, 20 feet telescope. A cluster of very compressed
“ stars, much accumulated in the middle ; 4 or 5 minutes in
“ diameter. ”

By the observation of the 10 feet telescope, the profundity of this cluster is of the 344th order. It is in the preceding branch of the milky way.

Observations of the 22d of the connoissance.

“ 1783, 7 feet telescope. 460 has not light enough to show
“ it; with 227, I see it very imperfectly. ”

“ 1801, 10 feet telescope. With 600 it is a cluster of stars. ”

“ 1783, small 20 feet telescope. With 350, all resolved
“ into stars. ”

“ 1784, 20 feet telescope. An extensive cluster of stars. ”

“ 1810, large 10 feet telescope. The stars are condensed
“ in the middle. The diameter is 8' 0"; the greatest conden-
“ sation is about 4' 0". ”

By the observation of the 10 feet telescope, the profundity of this cluster must be nearly of the 344th order. It is near the following branch of the milky way.

Observations of the 30th of the connoissance.

" 1794, 7 feet finder. It is but just visible."

" 1794, 7 feet telescope. It seems to be resolvable, but is too faint to bear a high power."

" 1810, 10 feet telescope. With 71, it appears like a pretty large cometic nebula, very gradually much brighter in the middle. 1783, with 250 it is resolved into very small stars."

" 1783, small 20 feet NEWTONIAN, 12 inch diameter. Power 200; it consists of very small stars; with two rows of stars, 4 or 5 in a line."

" 1783, large 20 feet NEWTONIAN. Power 120; by a drawing of the cluster, the rows of stars probably do not belong to the cluster."

" 1784, 1785, 1786, 20 feet telescope, power 157. A brilliant cluster."

" 1810, large 10 feet telescope. With 171 and 220 the diameter is 3' 5"; it is not round."

By the observation of the 10 feet telescope, the profundity of this cluster is of the 344th order.

Observations of the 33d of the connoissance.

" 1799, 10 feet finder. It is visible as a faint nebula."

" 1783, 1794, 7 feet telescope. With 75, it has a nebulous appearance; it will not bear 278 and 460, but with 120 it seems to be composed of stars."

" 1799, 1810, 10 feet telescope. The brightest part is resolvable; some of the stars are visible."

" 1805, 1810, Large 10 feet telescope. The condensation

for ascertaining the distances of clusters of stars, &c. 443

“ of the stars is very gradual towards the middle ; but with
“ the four powers 71, 108, 171, and 220, some nebulosity
“ remains. The stars of the cluster are the smallest points
“ imaginable. The diameter is nearly 18 minutes.”

The profundity of this cluster, by the observation of the 10 feet telescope, must be of the 344th order.

Observations of the 34th of the connoissance.

“ 1799, 7 feet finder. It is visible.”

“ 1783, 1794, 7 feet telescope. A cluster of stars ; with
“ 120, I think it is accompanied with mottled light, like stars
“ at a distance.”

“ 1784, 1786, 20 feet telescope. A coarse cluster of large
“ stars of different sizes.”

By the observation of the 7 feet telescope, the profundity of this cluster does probably not exceed the 144th order.

Observations of the 35th of the connoissance.

“ 1794, It is visible to the naked eye as a very small
“ cloudiness.”

“ 1783, 1794, 1801, 1813, 7 feet telescope. It is a rich
“ cluster of stars of various sizes.”

“ 1806, 10 feet telescope. There is no central contraction
“ to denote a globular form.”

“ 1783, 1785, 20 feet telescope. A cluster of pretty com-
“ pressed large stars.”

The profundity of this cluster does probably not exceed the 144th order. It is in the milky way.

Observations of the 53d of the connoissance.

“ 1813, 7 feet finder. It appears like a very small haziness.”

“ 1783, 7 feet telescope. With 460 the object is extremely faint. 1813, with 118 it is easily resolvable, and some of the stars may be seen.”

“ 1783, 10 feet telescope. With 250, I perceive 4 or 5 places that seem to consist of very small stars.”

“ 1784, 1786, 20 feet telescope. A globular cluster of very compressed stars.”

From the observation of the 7 feet telescope, it appears that the profundity of this cluster is of the 243d order.

Observations of the 55th of the connoissance.

“ 1783, small 20 feet telescope. With 250 fairly resolved into stars; I can count a great many of them, while others are too close to be distinguished separately.”

“ 1784, 1785, 20 feet telescope. A rich cluster of very compressed stars, irregularly round, about 8 minutes long.”

By the observation of the small 20 feet telescope, which could reach stars 38.99 times as far as the eye, the profundity of this cluster cannot be much less than of the 467th order: I have taken it to be of the 400th.

Observations of the 56th of the connoissance.

“ 1783, 7 feet telescope. A strong suspicion of its being stars.”

“ 1783, 1799, 10 feet telescope. 120 will not resolve it;

“ 240 wants light : 350 however shows the stars, but they
“ are so exceedingly close and small that they cannot be
“ counted.”

“ 1784, 1807, 20 feet telescope. A globular cluster of
“ very compressed small stars about 4 or 5 minutes in dia-
“ meter.”

“ 1805, 1807, large 10 feet telescope. With 171 it is 3' 36"
“ in diameter.”

The profundity of this cluster, by the observation of the
10 feet telescope, must be of the 344th order. It is near the
preceding branch of the milky way.

Observations of the 57th of the connoissance.

“ 1782, 7 feet telescope. I suspect it to consist of very
“ small stars ; in the middle it seems to be dark.”

“ 1783, 1805, 1806, 10 feet telescope. With 130 it seems
“ to be a rim of stars, but with 350 there remains a doubt.
“ It is a little oval ; the dark place in the middle is also oval ;
“ one side of the bright margin is a little narrower than the
“ other.”

“ 1784, 1799, 20 feet telescope. It is an oval with a dark
“ place within ; the light is resolvable. 240 showed several
“ small stars near, but none that seem to belong to it. It is
“ near 2 minutes in diameter.”

“ 1805, large 10 feet telescope. By a meridian passage of
“ 7 seconds of sidereal time, the diameter is 1' 28".4.”

By the observation of the 20 feet telescope, the profundity
of the stars of which it probably consists must be of a higher
than the 900th order ; perhaps 950.

Observations of the 62d of the connoissance.

" 1783, 10 feet telescope. With 250, a strong suspicion, amounting almost to a certainty, of its consisting of stars."

" 1785, 1786, 20 feet telescope. Extremely bright, round, very gradually brighter in the middle, about 4 or 5 minutes in diameter. 240 with strong attention showed the stars of it. The cluster is a miniature of the 3d of the connoissance."

By the 20 feet telescope, which at the time of these observations was of the NEWTONIAN construction, the profundity of this cluster is of the 734th order. It is in the preceding branch of the milky way.

Observations of the 67th of the connoissance.

" 1783, 7 feet telescope. A cluster of stars."

" 1809, 10 feet telescope. A cluster of very small stars."

" 1784, 20 feet telescope. A most beautiful cluster of stars; not less than 200 in view."

By estimation, the profundity of this cluster may be of the 144th order.

Observations of the 68th of the connoissance.

" 1786, 1789, 1790, 20 feet telescope. A cluster of very compressed small stars, about 3 minutes broad and 4 minutes long. The stars are so compressed, that most of them are blended together."

Probably the stars of this cluster might be perceived by a 10 feet telescope, so that its profundity may be of the 344th order.

Observation of the 69th of the connoissance.

“ 1784, 20 feet telescope. Very bright, pretty large, easily
“ resolvable, or rather an already resolved cluster of minute
“ stars. It is a miniature of the 53d of the connoissance.”

By this observation, the profundity of the cluster must be
of the 734th order.

Observations of the 71st of the connoissance.

“ 1794, 7 feet telescope. With 120 and 160 the stars of it
“ become just visible.”

“ 1783, 1799, 1810, 10 feet telescope. A cluster of stars
“ of an irregular figure.”

“ 1784, 1799, 1807, 20 feet telescope. It is situated in the
“ milky way, and the stars are probably in the extent of it;
“ it is however considerably condensed; about 3 minutes in
“ diameter.”

“ 1805, large 10 feet telescope. An irregular cluster of
“ very small stars, 2' 35" in diameter.”

By the observation of the 7 feet telescope, the profundity
of this cluster is of the 243d order. It is in the following
branch of the milky way.

Observations of the 72d of the connoissance.

“ 1805, 7 feet telescope. With a power of 80 the stars
“ may just be perceived.”

“ 1783, 1810, 10 feet telescope. With 150 fairly re-
“ solved.”

“ 1784, 1788, 20 feet telescope. A cluster of very small
“ stars.”

" 1810, large 10 feet telescope. A globular cluster; its diameter is $2' 40''$."

" 1810, 40 feet telescope. A beautiful cluster of stars."

By the observation of the 7 feet telescope, the profundity of this cluster must be of the 243d order.

Observations of the 74th of the connoissance.

" 1783, 1784, 7 feet telescope. With 100 and 120 it is a collection of very small stars; I see many of them."

" 1799, 1801, 10 feet telescope. Several of the stars are visible; it is a very faint object."

" 1784, 20 feet telescope. Some stars are visible in it; the edges are not resolvable."

" 1805, 1810, large 10 feet telescope. With 108 it consists of extremely small stars, of an irregular figure; a very faint object of nearly 12 minutes in diameter."

" 1799, 40 feet telescope. Very bright in the middle, but the brightness is confined to a very small part."

By the observation of the 7 feet telescope, the profundity of the nearest part of this cluster must be of the 243d order, but most probably a succession of more distant stars was seen in the larger telescopes.

Observations of the 75th of the connoissance.

" 1799, 7 feet finder. It is but just visible."

" 1799, 7 feet telescope. There is not the least appearance of its consisting of stars, but it resembles other clusters of this kind, when they are seen with low space-penetrating and magnifying powers."

“ 1810, 10 feet telescope. With 71 it is small and
“ cometic.”

“ 1784, 1785, 20 feet NEWTONIAN. Easily resolvable ;
“ some of the stars are visible.”

“ 1810, 20 feet front view. It is a globular cluster.”

“ 1799, 1810, large 10 feet. Its diameter with 171 is
“ 1' 48" ; with 220 it is 2' 0".”

By the observation of the 20 feet NEWTONIAN telescope, the
profundity of this cluster must be of the 734th order.

Observations of the 77th of the connoissance.

“ 1783, 7 feet telescope. An ill defined star, surrounded
“ by nebulosity.”

“ 1801, 1805, 1809, 1810, 10 feet telescope. It has
“ almost the appearance of a large stellar nebula.”

“ 1783, 1785, 1786, 20 feet telescope. Very bright ; an
“ irregular extended nucleus with milky chevelure, 3 or 4
“ minutes long, near 3 minutes broad.”

“ 1801, 1805, 1807, large 10 feet telescope. A kind of
“ much magnified stellar cluster ; it contains some bright
“ stars in the centre. With 171 its diameter is 1' 17" ; with
“ 220 it is 1' 36".”

From the observations of the large 10 feet telescope, which
has a gaging power of 75.82, we may conclude that the pro-
fundity of the nearest part of this object is at least of the
910th order.

Observations of the 79th of the connoissance.

“ 1783, 7 feet telescope. With 57 nebulous ; with 86 a
“ strong suspicion of its being stars.”

" 1799, 10 feet telescope. 300 shows the stars of it with difficulty."

" 1784, 20 feet telescope. A beautiful cluster of stars, nearly 3 minutes in diameter."

" 1806, large 10 feet telescope. A globular cluster, the stars of which are extremely compressed in the middle; with 171 and 220 the diameter is $2' 50''$, but the lowness of the situation probably prevents my seeing the whole of its extent."

By the observation of the 10 feet telescope the profundity of the cluster is of the 344th order.

Observations of the 80th of the connoissance.

" 1784, 1786, 20 feet telescope. A globular cluster of extremely minute and very compressed stars of about $\frac{3}{4}$ or $\frac{4}{5}$ minutes in diameter; very gradually much brighter in the middle; towards the circumference the stars are distinctly to be seen, and are the smallest imaginable."

The profundity of this cluster is probably not much less than of the 734th order.

Observations of the 92d of the connoissance.

" 1799, 7 feet finder. It may just be distinguished; it is but very little larger than a star."

" 1783, 2 feet sweeper. With 15 it appears like a clouded star."

" 1783, 7 feet telescope. With 227 resolved into very small stars; with 460 I can count many of them."

" 1799, 10 feet telescope. With 240 the stars are much condensed in the centre."

“ 1783, 1787, 1799, 20 feet telescope. A brilliant cluster; about 6 or 7 minutes in diameter.”

“ 1805, large 10 feet telescope. The most condensed part is 3' 16" in diameter.”

The profundity of this cluster, by the observation of the 7 feet telescope, is of the 243d order.

Observations of the 97th of the connoissance.

“ 1799, 7 feet finder. The object is not visible in it.”

“ 1789, 20 feet telescope; considerably bright, globular, of equal light throughout, with a diminishing border of no great extent. About 3 minutes in diameter.”

“ 1805, large 10 feet telescope. The constellation being too low it had the appearance of a faint nebula.”

From the observation of the 20 feet telescope, it appears that the profundity of this object is beyond the gaging power of that instrument; and as it must be sufficiently distant to be ambiguous, it cannot well be less than of the 980th order.

III. Of a method to represent the profundity of celestial objects in space by a diagram.

In order to represent the profundity of celestial objects in space, I shall have recourse to the construction of an astronomical globe, on the surface of which the situations of the heavenly bodies are pointed out to us in the given two dimensions of right ascension and polar distance; but as their distance from an eye placed in the centre of the globe cannot be expressed by their situation on the surface, I shall endeavour to show that this deficiency may be artificially

supplied in a figure representing such a globe, by the addition of lines that are of a length which is proportional to the diameter of it.

It has been shown in my last paper, that all the stars which may be seen in the clearest nights, are probably contained within a globular space, of which the radius does not exceed the 12th order of distances; I shall therefore suppose the circle *c* in the centre of fig. 1, [Plate XXI.] to represent a celestial globe, containing all the stars that are generally marked on its surface; their arrangement within this globular space, however, must be supposed to be according to their order of distances, the stars of the first order being placed nearest the centre, and those of the 2d, 3d, and 4th, &c. gradually farther off; but they must all be placed in their well ascertained directions, so that a line from the centre drawn through any one of them may come to the surface at the place where its situation is marked.

According to this assumption it follows, that all those celestial objects which are farther than the 12th order of distances from the centre, must be represented as being at the outside of the globular space; but as our celestial globes represent not only the situation of the stars of the heavens, but give us also many additional objects, such as clusters of stars, nebulae, and the milky way, it is evident that the point where the line of sight from the centre to any one of these distant objects, leaves the surface of the globular space, is ascertained; and since any celestial object not inserted on our globes, of which the right ascension and polar distance are given, may be easily added, the position of the visual ray directed to such an object will thereby also be determined.

In my last paper I have drawn the attention of astronomers to the condition of the milky way, as being the most brilliant, and beyond all comparison the most extensive sidereal system; and have also shown that the globular space containing all our visible stars, is situated within its compass; I shall therefore now make the plane of it the principal dimension of my figure; then if the line ab represent this plane, a perpendicular drawn from the centre c of the figure to d and to e , will be directed towards the north and south poles of it, and the situation of the globular space in the figure will be like that of a celestial globe adjusted to the latitude of 30 degrees, having the milky way in the horizon, the 190th degree of right ascension in the meridian, and the 60th degree of north polar distance in the zenith.

From this description of the arrangement of the stars within the globular space, and its situation in the plane of the milky way, it is evident that, having already an expression for the position of a celestial object in two dimensions, the addition of the third, which is its profundity or central distance, may be represented by a line of a length that is proportional to the diameter of the globular space; and if this line be a continuation of the direction in which the object is seen from the centre, its termination will show the real place of the object, and point out its situation with respect to the great sidereal stratum of the milky way.

An observer who looks at a celestial globe, and wishes to see the angle of the direction of the line in which an object is seen from the centre, will for this purpose turn the globe horizontally till the plane of the azimuth circle is at right angles to the line in which he looks at it; or, if more con-

venient, he will change his position by going round the globe till he comes to the situation in which this angle will appear of its true magnitude.

In illustration of this, let NESW, figure 2, [Pl. XXI.] be the circle on which the azimuths of celestial objects are to be reckoned, and let the meridional line NS pass through the 190th degree of right ascension at S; then will the numbers at the circumference of the circle point out the degrees, and the quadrant of the azimuth of the situation in which any object is to be seen when referred to the milky way. The particular use of this azimuth circle will appear, when the construction of the figure which expresses the profundity of the clusters of stars, of which I have given the observations, has been explained.

Having fixed upon the plane of the milky way as the region of the heavens to which the situation of the clusters of stars is to be referred, their right ascension and polar distance, which are required for this purpose, must be reduced to this plane; and will appear under the denomination of elevation and azimuth. The elevation from the plane of the milky way will be either north or south, and the azimuths in either the northern or southern hemisphere of it, will be in the north-east, south-east, north-west, and south-west quadrants. In order to make this reduction, we have the construction of the triangle ABC, figure 3, in which A is the pole of the heavens; B the north pole of the milky way, and C the situation of the cluster of stars; and there is given the side AB, which is the distance of the two poles; the side AC, which is the polar distance of the cluster, and the angle A, which is the difference between the right ascension of the

pole of the milky way and that of the cluster of stars. From these data we find the side BC, the complement of which is the angle of the elevation of the cluster; and the angle ABC, or its supplement CBD, which gives the degree and the quadrant of the azimuth of the cluster. When to these two particulars the profundity of a cluster is added, we have its local situation, with regard to the plane of the milky way, in the required three dimensions of space.

The following table is the result of a set of calculations made for the purpose of obtaining the above mentioned particulars.

Clusters of stars taken from my catalogues.				
Class No.	Profundity.	Elevation.	Azimuth.	Point of sight.
VI. 7	734	76° 58' N	31° 43' SE	58° 17' SW
	734	73 25 N	87 2 SE	2 58 SW
	734	14 11 N	48 42 SE	41 18 SW
11	734	8 35 N	55 10 SE	34 50 SW
12	466	6 26 N	54 30 SE	35 30 SW
17	600	2 52 N	63 49 NW	26 11 SW
20	734	87 39 S	10 57 NW	79 3 SW
26	900	0 5 S	35 38 NW	54 22 SW
35	900	0 27 N	1 55 NE	88 5 SE
38	900	4 31 S	77 5 NE	12 55 SE
41	900	32 55 N	16 9 NE	73 51 SE
IV. 63	900	59 47 N	10 23 NE	79 37 SE

Clusters of stars taken from the connoissance.				
1	980	4° 42' S	61° 24' NW	28° 36' SW
2	243	35 29 S	68 17 NE	21 43 SE
3	243	78 29 N	89 38 NE	0 22 SE
4	344	14 31 N	47 41 SE	42 19 SW
5	243	45 36 N	59 27 SE	30 33 SW
9	344	9 35 N	62 19 SE	27 41 SW
10	243	22 11 N	71 28 SE	18 32 SW
11	144	3 10 S	84 21 SE	5 39 SW
12	186	25 26 N	71 57 SE	18 3 SW
13	243	41 19 N	65 31 NE	24 29 SE
14	900	14 6 N	77 48 SE	12 12 SW
15	243	26 38 S	57 3 NE	32 57 SE
19	344	7 56 N	53 51 SE	36 9 SW
22	344	8 45 S	67 2 SE	22 58 SW
30	344	47 26 S	86 5 SE	3 55 SW
33	344	29 25 S	10 37 NW	79 23 SW
34	144	13 48 S	20 33 NW	69 27 SW
35	144	3 13 N	63 58 NW	26 2 SW
53	243	77 58 N	28 6 SE	61 54 SW
55	400	24 19 S	66 30 SE	23 30 SW
56	344	8 59 N	60 43 NE	29 17 SE
57	950	16 51 N	61 28 NE	28 32 SE
62	734	5 54 N	50 29 SE	39 31 SW
67	144	31 44 N	83 4 SW	6 56 SE
68	344	34 19 N	3 1 SW	86 59 SE
69	734	11 35 S	59 6 SE	30 54 SW
71	243	4 10 S	66 6 NE	23 54 SE
72	243	32 58 S	86 40 NE	3 20 SE
74	243	43 53 S	15 28 NW	74 32 SW
75	734	26 29 S	78 9 SE	11 51 SW
77	910	50 32 S	47 36 NW	42 24 SW
78	344	29 25 S	76 47 SW	13 13 SE
80	734	18 41 N	48 39 SE	41 21 SW
92	243	35 33 N	55 50 NE	34 10 SE
97	980	58 52 N	26 5 NW	63 55 SW

In order to explain the construction of the table, and the use that is to be made of it when the situation of any one of the clusters delineated in figure 1 is to be examined, I shall take the first cluster it contains for an example.

The first column points out the class and number, where the clusters taken from my catalogues are to be found, and only the number of those that are taken from the *Connoissance des Temps* for 1784. In the figure, the place of the cluster whose situation is to be examined is distinguished by the same mark as in the table namely VI, 7.

The second column contains the distance of the same cluster from an eye placed in the centre of the globular space, the profundity of which is 734, as determined by the observations that have been given. In the figure it is expressed by the length of the line *c* VI, 7 drawn from the centre of it to the cluster, whose length is 734, the radius of the circle representing the globular space being 12.

The third column gives the angle of elevation of the cluster, which in the present instance is $76^{\circ} 58'$ above the northern plane of the milky way. In the figure it is expressed by the central meeting of the lines *b c* and *c* VI, 7: one of which denotes this plane, and the other the profundity of the cluster.

To find the quantity of this angle, it is necessary to have the right ascension and polar distance of the cluster; and here it will be proper to notice that I have deduced these requisites from my own observations of the clusters, brought to the beginning of the year 1800. Then to find the elevation of the present cluster by the method which has been explained, we have in figure 3, the side $AB = 60^{\circ}$: the side

$AC = 71^{\circ} 17'$, being the polar distance of the cluster ; and the angle $A = 6^{\circ} 56' 45''$, being the right ascension of the cluster $196^{\circ} 56' 45''$ minus 190° . By these quantities we find $BC = 13^{\circ} 2' 28''$, and its complement $76^{\circ} 57' 32''$, which is the required elevation of the cluster VI, 7.

The fourth column assigns the azimuth of the cluster ; and as the degrees of the quadrants of the azimuth circle in figure 2, are numbered one from the south the other from the north, the letter S is prefixed to E, to show that the degrees of it are to be looked for in the south-east quadrant ; the quantity of the angle, in consequence of the foregoing calculation, is easily obtained ; for as we now already have the side BC, the opposite angle A, and the side AC, we find the supplemental angle CBD, which gives the azimuth $31^{\circ} 43' 9''$. By this result the situation of the direction, in which an observer in the centre of the globular space must look to see the cluster, is determined.

The fifth column contains the point of sight, or situation in which the eye of an observer should be placed, when, by the assistance of a celestial globe, the profundity of any cluster marked in the figure is to be examined. This point, for the cluster VI, 7, is $58^{\circ} 17'$ south-west, which denotes that the globe must be turned horizontally till the 58th degree of the south-west quadrant directly faces the observer, or that, by changing his situation, he must place himself so as to face the globe in the assigned position.

I have called the construction of the figure which gives the profundity of the clusters, an artificial one ; because, as soon as the celestial globe is brought into the situation where it can be seen from the tabular point of sight, the figure will

always be found already prepared to show by inspection the azimuth, the elevation and the profundity of the cluster under examination; for as the globe, which in its adjusted situation has the azimuth of the cluster VI, 7, at right angles to the line of sight, so the globular space in the centre of the figure being supposed similarly arranged, has the tabular azimuth $31^{\circ} 43'$ SE also at right angles to the line drawn to the figure, when seen from the point of sight $58^{\circ} 17'$ SW.

The direction from the centre of the globe to the place on its surface where the cluster is inserted, is also preserved in the continuation of it beyond the surface of the globular space, by the angle of its elevation $76^{\circ} 58'$ above the northern plane of the milky way.

The profundity of the cluster, as has already been noticed, is expressed by the continuation of the line of elevation to 734 such parts as the radius of the globular space contains 12; and it may not be amiss, by way of assisting our conception of the vast distance of the situation at which this cluster is placed, to state, that if a line directed to it were added to an 18 inch globe, supposed to contain all the visible stars of the heavens, its length to express this distance would be above 45 feet.

This figure which, from its construction, represents all the different aspects in which a celestial globe should be seen, when its horizontal position for any cluster is adjusted by the foregoing table, has the imperfection that, on account of the different azimuths of their situation, they cannot all be collected into one perspective view; but as it affords the means of examining them separately, which may even be done without the assistance of the globe, this inconvenience

is compensated by the advantage it has of showing all the angles of elevation, and the comparative lengths of the lines expressing the profundity of the clusters in their true magnitude, which an orthographic projection of their situation could not have done.

IV. *Of ambiguous celestial objects.*

When the nature or construction of a celestial object is called ambiguous, this expression may be looked upon as referring either to the eye of the observer, or to the telescope by which it has been examined. In the foregoing observations we find that the 11th, 13th, 15th, and 35th of the connoissance, when they are at a sufficient altitude for the purpose, may be seen by the eye; but as, without artificial vision, they appear only under the semblance of very small, faint cloudy spots, we should not be able to decide whether they were of a nebulous or sidereal condition, if we were not informed by the telescope that they are brilliant clusters of stars; the eye therefore sees them as ambiguous objects.

If these objects are ambiguous when only viewed by the unassisted eye, there are others that will appear to be so, when they are seen through such small telescopes as are generally attached to large ones, and are called finders, because they point out objects that are not visible to the eye. With regard to these finders, I have occasionally used them of different sizes and constructions; but from experience I can say, that a small one of a most simple composition, with a power of penetrating into space of about four times that of the eye, has generally been sufficient for all the purposes of a 7 or 10 feet telescope; because these instruments may

easily be made to act as finders to themselves, by using a double eye glass with a large field of view and a small magnifying power. It is indeed very obvious that when a small telescope, acting as a finder to a larger one, has not sufficient light to show the objects we look for, a more powerful one must be used. In this manner I have often been obliged to have recourse to a 10 feet reflector as a finder, to point out the situation of an object to be viewed in the 20 feet telescope.

It may have appeared singular, that among the observations which have been given, there are many that were made by the 7 and 10 feet telescope finders, but the important use of these observations will appear in the consequences that may be drawn from them; for the clusters of stars, No. 2, 3, 5, 12, 30, 33, 34, 53, 75, and 92 of the connoissance were all to be seen in these finders; they were, however, not seen as clusters of stars, but as ambiguous objects. No. 12, 30, 34, and 75 were but just to be perceived; No. 2 and 92 appeared like stars with rather a large diameter; No. 3 and 5 like hazy stars; No. 33 and 53 like small hazinesses or nebulosities; and yet they were all proved by the telescopes in which they were critically examined to be clusters of stars. If then a cluster of stars in a very small telescope will appear like a star with rather a larger diameter than stars of the same size generally have, we shall certainly be authorised to conclude, that an object seen in a larger and more perfect telescope as a star with rather a larger diameter, is also an ambiguous object, and might possibly be proved to be a cluster of stars, had we a superior instrument by which we could examine its nature and construction.

This seems to throw some light upon a species of objects called stellar nebulæ, one hundred and forty of which have been inserted in my catalogues. For as it has just been mentioned that a 10 feet telescope may become a finder to a 20 feet one, the 20 feet telescope itself will be but a finder to objects that are so far out of its reach as not to appear otherwise than ambiguous; nay, the 40 feet telescope, when it is but just powerful enough to show the existence of an object which decidedly differs from the appearance of a star, may then truly be called a finder.

V. The milky way, at the profundity beyond which the gaging powers of our instruments cannot reach, is not an ambiguous object.

Celestial objects can only be said to remain ambiguous, when the telescopes that have been directed to them leave it undetermined whether they are composed of stars or of nebulous matter. Six observations of different parts of the milky way, relating to this subject, have already been given in my last paper,* to which the following four may be added.

Dec. 27, 1786. Right ascension $6^h 42'$. Polar distance $88^\circ 38'$. There are 116 stars in the field of view, besides many too small for the gage.

Sept. 21, 1788. Right ascension $21^h 29'$. Polar distance $41^\circ 1'$. There are about 360 stars in the field of view, but most of them are so small that it requires the utmost attention to see them.

Sept. 27, 1788. Right ascension $21^h 17'$. Polar distance

* See Phil. Trans. for 1817, pages 325, 326, and 329.

for ascertaining the distances of clusters of stars, &c.

52° 50'. With 157 there are small stars with suspected nebulosity; 300 shows a great many smaller stars intermixed with the former.

Sept. 11, 1790. Right ascension 19^h 50'. Polar distance 47° 0'. About 240 stars in the field of view, with many too small to be counted.

In these ten observations the gages applied to the milky way were found to be arrested in their progress by the extreme smallness and faintness of the stars; this can however leave no doubt of the progressive extent of the starry regions; for when in one of the observations a faint nebulosity was suspected, the application of a higher magnifying power evinced, that the doubtful appearance was owing to an intermixture of many stars that were too minute to be distinctly perceived with the lower power; hence we may conclude, that when our gages will no longer resolve the milky way into stars, it is not because its nature is ambiguous, but because it is fathomless.

VI. Of the assumed semblance of clusters of stars, when seen through telescopes that have not light and power sufficient to show their nature and construction.

The variety of telescopes used in the long series of observations that have been given, will afford us many instances to ascertain the various deceptive appearances that clusters of stars may put on when they are observed with an inadequate apparatus.

An examination of some particulars relating to this subject may assist us to ascertain in what class we ought probably to

place the numerous observations of ambiguous objects that in my sweeps of the heavens were seen by the 20 feet telescope; and having already compared the different forms under which clusters of stars appeared in the finders of the instruments, I shall now also notice how they were seen in the gradually larger telescopes.

In the 2 feet NEWTONIAN sweeper,

No. 92 of the connoissance appeared like a clouded star, with a magnifying power of 15. No. 2, with a power of 24, appeared like a telescopic comet.

In the 7 feet telescope,

No. 77 was like an ill defined star, surrounded by nebulosity. No 79, with a power of 57, appeared nebulous. With 460 No. 3 could hardly be seen, for want of light. No. 10, with 227, could not be resolved into stars, for want of power. With 460 No. 22 wanted light, and with 227 it wanted power. With a magnifier of 171 No. 33 had a nebulous appearance. No. 1 was seen as light without stars.

In the 10 feet telescope,

The light of No. 19 appeared mottled. With a power of 71 No. 30 appeared like a pretty large cometic nebula, very gradually much brighter in the middle. With the same power No. 75, was small and cometic. No. 77 had nearly the appearance of a large stellar nebula.

In the large 10 feet telescope,

No. 97, being too low for examination, had the appearance of a faint nebula.

The numerous ambiguous objects that have been seen in the 20 feet telescope do not properly come under this head ; for as none of them have been critically examined by superior telescopes, they must still remain ambiguous; and it is for the purpose of being able to form some probable conjecture about the nature of these doubtful objects, that the foregoing results of the appearance of such as have been ascertained to be clusters of stars, have been pointed out.

It would be far too extensive to enter into particular applications, I shall therefore confine myself to a few general remarks. In the depth of the celestial regions, we have hitherto only been acquainted with two different principles, the nebulous and the sidereal. The light of the nebulous matter is comparatively very faint, and, except in a few instances, invisible to the eye. It is also in general widely diffused over a great expanse of space, in which, by an increase of faintness, it generally escapes the sight : the light of stars on the contrary, is comparatively very brilliant, and confined to a small point, except when many of them are collected together in clusters, when their united lustre sometimes takes up a considerable number of minutes of space ; but in this case the stars of them may be seen in our telescopes, and by the observations that have been given, it appears that when they are viewed with instruments gradually inferior to those which prove them to be clusters of stars, their diameters, seen with less light and a smaller magnifying power, are

generally contracted; a globular cluster is reduced to a cometic appearance; to an ill defined star surrounded by nebulosity, and to a mere small star with rather a larger diameter than stars of the same size generally have. In consequence of these considerations, it seems to be highly probable that some of the cometic, many of the planetary, and a considerable number of the stellar nebulæ, are clusters of stars in disguise, on account of their being so deeply immersed in space, that none of the gaging powers of our telescopes have hitherto been able to reach them. The distance of objects of the same appearances, but which are of a nebulous origin, on the contrary, must be so much less than that of the former, that their profundity in space may probably not exceed the gooth order.

VII. *Of the extent of the power of our telescopes to reach into space, when they are directed to ambiguous celestial objects.*

The method of equalising the light of stars on which the gaging power of telescopes has been established, may also be applied to give us an estimate of the extent of their power to reach ambiguous celestial objects.

When the united light of a cluster of stars is visible to the eye, there will then be a certain maximum of distance to which the same cluster might be removed so as still to remain visible in a telescope of a given space-penetrating power; and if the distance of this cluster can be ascertained by the gaging power of any instrument that will just show the stars of it, the order of the profundity at which the cluster could still be seen as an ambiguous object, may be ascertained

by the space-penetrating power of the telescope through which it is observed. But as the aggregate brightness of the stars depends entirely on their number and arrangement, this method can only be used with clusters of stars that have been actually observed.

The 35th of the connoissance, for instance, being visible to the eye as a small cloudiness, its profundity in space was, by an observation of the 7 feet telescope, shown probably not to exceed the 144th order; then, as the stars that enter into the composition of this cluster are of such an arrangement that their united lustre may be seen by the eye at the distance of the 144th order, the 10 feet telescope, by which this cluster was viewed, having a power of penetrating into space 28.67 times that of the eye, would be able to show this cluster as a small cloudiness, if it were removed to the distance of the 4128th order. The 20 feet NEWTONIAN telescope, in which it was also observed, having a space-penetrating power 61.16 times that of the eye, would still be sufficient to discover it as an ambiguous object, if it were removed to the distance of the 8809th order.

To investigate how far the 15th cluster, which is also visible to the eye, might be removed, so as still to be seen in the front view of the 20 feet telescope, we find, by inspecting the table in which the profundities are given, that the eye can reach it at the distance of the 243d order; therefore this telescope, with a power 75.08 times that of the eye, would still be able to show it at the distance of the 18244th order, and being a globular cluster, its appearance would be that of a small star with rather a large diameter.

As there are but few clusters of stars that can be seen by the eye, the observations of their visibility in the finders of telescopes, and their appearance in them, are of eminent use in ascertaining the distance at which we can expect to see celestial objects in large telescopes; when, therefore, a cluster of stars cannot be seen by the eye, its visibility in the finder must first of all be reduced to the standard of the eye. I have already noticed that the power of my finders to show stars, has generally been about four times that of the eye; then, as they would show a star at the distance of the 48th order, a celestial object, situated at this distance, would require to be brought to one quarter of that distance to become visible to the eye.

The 2d cluster of the connoissance, for instance, was seen in a finder with the above mentioned power, and its profundity having been ascertained to be 243, we may conclude that it would be visible to the eye, if it were only of the 60.75th order; this being admitted, it will follow that the 20 feet telescope would still show this cluster of stars as an ambiguous object, if it were removed to the 4561st order; and with a space-penetrating power of 191.69, the 40 feet telescope, by which it was also observed, would have shown this cluster under the semblance of a star that might be distinguished from others by having rather a larger diameter, if it had been at the distance of the 11645th order.

In the foregoing instances, I have assigned the extent of the power to reach celestial objects, as it is in the same instruments whereby they were observed, but this is not a necessary condition; for when the visibility, and the particu-

lar manner of its appearance of any cluster of stars in a finder or in a small telescope of any known gaging power is ascertained; and when also by any superior instrument its profundity in space has been assigned, so that it may be reduced to the station at which it would be visible to the eye, it may then be viewed with any telescope of which the space-penetrating power is known; and if we put e for the power of the eye, f for that of the telescope which acts the part of the finder, p for the ascertained profundity of the cluster, and S for the space-penetrating power of a superior telescope, then will the extent E of this telescope to reach the same cluster, as an ambiguous object of any required appearance, be had by the formula $E = \frac{e p S}{f}$.

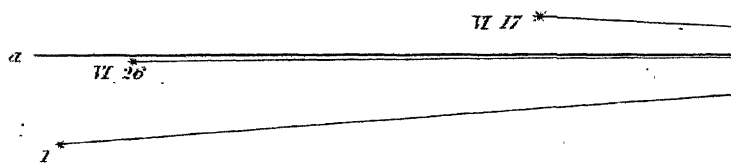
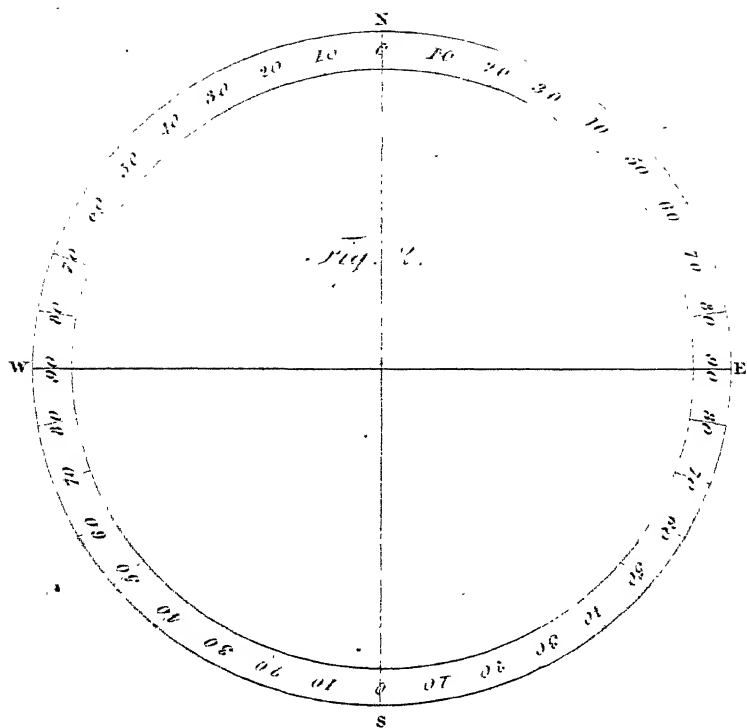
It will not be necessary to calculate, by this formula, the order of distances at which in large telescopes some of the clusters of stars would be seen like telescopic comets; others as large stellar nebulae, and others again as ill defined stars surrounded by nebulosity, as all these appearances must fall within the compass of the full stretch of their power; I shall therefore only add a calculation of the ambiguous visibility of one of the very distant clusters of stars.

The 75th of the connoissance is not visible to the eye, but may be seen in the finder; and the telescopic observations of it have ascertained its profundity to be of the 734th order; the station to which it should be brought, that it might be visible to the eye, is therefore of the 183.5th order. From this it follows, that with any telescope which has the space-penetrating power of the front view of my 20 feet reflector, this cluster might still be perceived if it were removed to the

distance of the 13707th order ; and that the 40 feet telescope, which in this case would really act the part of a finder, would still show this cluster of stars as an ambiguous object at a profundity in space amounting to the 35175th order.

W. HERSCHEL.

Slough, near Windsor.



A

Fig. 3.

XXIV. *On the structure of the poisonous fangs of serpents.* By
Thomas Smith, Esq. F. R. S.

Read June 4, 1818.

WHEN the poisonous fangs of serpents are attentively examined, a slit or suture may be observed extending along the convex side, from the foramen at the base to the aperture near the point. (Plate XXII. A. B. C. D.) This is a consequence of an unusual, and hitherto, I believe, entirely unnoticed structure, resulting from the mode of formation of the tube through which the poison flows.

My attention was called to this structure, by having lately received from my friend Mr. HERBERT RYDER, the assay master to the mint at Madras, the bones of the skull of a cobra de capello. I had some years since noticed the slit running along the convex side of the fang, in making a preparation of the head of the common viper of this country, in which it is distinctly seen when magnified; nevertheless, it seems to have been overlooked by all the numerous authors who have written upon the subject of the venomous fangs of the viper, and who, as far as structure is concerned, do not appear to have advanced beyond PLINY, to whom, and even anterior to whose time, the circumstance of their being tubular was well known.

All teeth being formed from a pulp, which has the shape that the tooth itself is destined to retain, it has probably been imagined that the tube of the poisonous fangs of serpents

was produced by a perforation passing through the pulp ; this is not, however, the case, the tube being completely external, and formed by a deep longitudinal depression on the surface of the pulp.

In order to render this more clear, I must here observe that a slight longitudinal furrow, or depression, is to be seen on all the teeth of the cobra de capello ; on those which are nearest to the poisonous fangs it is most evident, and occupies the convex side of their curvature ; it however is confined entirely to the parietes of the tooth, and does not at all affect the form of its cavity.

But in the poisonous fangs, this depression is sunk deep into the substance of the tooth, and occupies a portion of the space, which in the others is allotted to the cavity which contains that part of the pulp which remains when the tooth is completely formed ; and the edges of the depression being brought together along the greater part of the tooth, form the slit or suture that I have before described, but, being kept at a distance at both extremities, there results a foramen at the base and at the apex.

That this is a correct view of the mode in which the poisonous tube is formed, receives additional support from what I have observed in a species of the genus *hydrus*, of SCHNEIDER. In this serpent, as in many others nearly allied to it (les hydres of M. CUVIER), there are simple teeth on the same bone which supports the poisonous fangs. These teeth so much resemble the fangs, that it requires a very close investigation to distinguish between them ; and this arises from the simple tooth having not only a longitudinal furrow exactly resembling the edges of the slit of the poisonous

fang, but also a very visible cavity at the base, where the foramen occurs in the others; and I have even found a fine tube in a tooth of this sort; it was however confined to the parietes, and did not affect the cavity of the tooth.

To this gradation from a slight superficial furrow to a deep depression, may be added the fact, that no traces of either are observable in the teeth of those serpents which are not armed with venomous fangs: this I found to be the case in a large species of boa.

As a consequence of the structure that I have described, if a horizontal section be made of a poisonous fang, in which the edges of the longitudinal depression are rounded, we shall have a cylindrical cavity (the poison tube) nearly surrounded by a semilunar one (the cavity which contains the pulp). This is shown in the annexed drawings of the fangs of the cobra de capello. (Pl. XXII. E. F. G. H.)

If, however, the edges of the depression should be angular (as in the rattle snake), the horizontal section shows a figure somewhat different, the poison tube being more completely surrounded by the cavity which contains the pulp. This is shown in the drawing by the section of a fang of an unknown species of serpent, which has exactly the same form as that of the rattle snake, but is twice as large. (Pl. XXII. I. K.)

In sections taken at different parts of the fang, the proportions between the poison tube and the cavity which contains the pulp will be different; the latter greatly increasing towards the base of the tooth; and near the apex the poison tube only will be seen, the fang at that part being solid. In a section also of a completely formed fang, the poison tube, at its anterior part, will be closely invested by the thickened

parietes of the cavity which contains the pulp; this cavity however is never obliterated, but exists in all the teeth of serpents, even when they have arrived at their full growth.

In the fangs, when completely formed, the edges of the slit, or suture, are frequently soldered together; when they are angular, so large a surface comes in contact, that they appear to be united by bony matter; in the cobra de capello, where they are rounded, though in very close contact, they do not cohere. In the viper, the slit seems filled up by the enamel, which being nearly transparent, a bristle in the poison tube may be seen through it, and causes an appearance as if the slit was open.

In the first case, therefore, there is no channel observable on the exterior of the tooth; the line of junction, however, of the edges of the slit is very distinctly marked: in the cobra de capello there is an external furrow from the foramen of the base to that of the apex, owing to the edges of the slit being rounded; the same is the case in those species of hydrus that I have examined.

I should observe, that the poison tube is not coated with enamel; for the membrane or capsule in which the tooth is formed, and from the inner surface of which it is well known that the enamel is deposited, does not pass between the edges of the slit into the poison tube; as, however, it passes over the slit, it will cover it with enamel, and in some cases, by that means alone, the edges become soldered together.

As some excuse for the errors which may be found in this paper, I must observe, that many of my observations have been confined to small teeth of a species of hydrus, which I was therefore obliged to dissect under the microscope.

I have to thank Sir EVERARD HOME for the great interest that he has taken in the object of my enquiry, and for the assistance which he has afforded me; on the value of which it would be needless to enlarge before the Members of this Society.

The drawings annexed to the paper will sufficiently attest my obligations to Mr. CLIFT. I owe much to him, in addition, for the zeal with which he exhibited to me every thing in the Museum of which he has the custody, that was likely to promote my views, and for information upon several points, which was required in the progress of the investigation.

DESCRIPTION OF PLATE XXII.

a, b, c, d, are representations of the poisonous fangs of the cobra de capello, in four stages of their growth.

A, B, C, D, are magnified representations of the same.

A, is a full grown fang firmly fixed to the bone.

B, is not quite perfect, the lower part of the foramen at the base not being yet formed.

C, in this a very small part of the foramen is formed.

D, the part of the tooth above the foramen alone appears.

E, F, G, are end views of B, C, D, showing the poison tube nearly surrounded by the cavity which contains the pulp, and the proportions between them, at three different parts of the tooth.

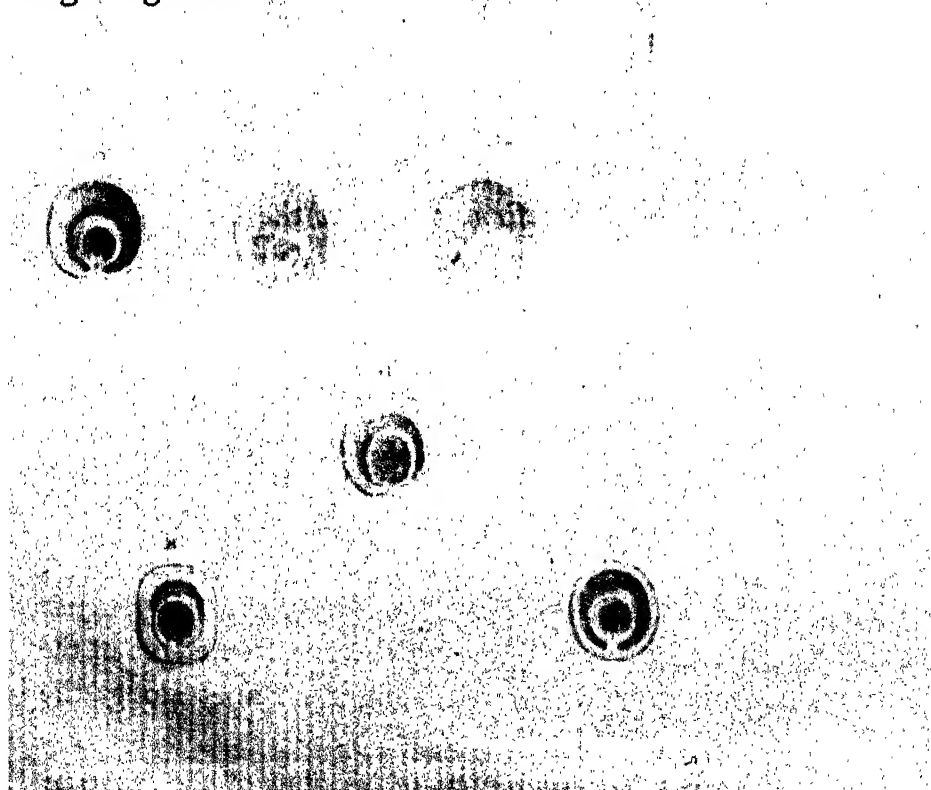
H, a section made by sawing the full grown fang A, just above the lower foramen, showing the rounded edges of the slit, which consequently leave a slight channel along the tooth.

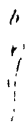
I, K, magnified representations of sections of the fangs of

an unknown species of serpent, which have exactly the same form as those of the rattle snake.

I, is a section of a young fang taken about the middle: in this stage of growth the cavity which contains the pulp almost entirely surrounds the poison tube, and the edges of the depression which form the suture are seen to be angular, and present so large a surface to each other, that the suture is completely filled up even in this early stage of growth.

K, is a section of a full grown fang of the same species of serpent at the same part as the preceding. Here the cavity of the pulp is seen greatly contracted from the more advanced stage of growth.





XXV. *On the parallax of α Aquilæ.* By John Pond, F. R. S.
Astronomer Royal.

Read April 16, 1818.

THE telescope erected for the investigation of the parallax of α Aquilæ, exactly resembles in its construction that which was employed for the observations of α Cygni. It has an achromatic object glass of ten feet focal length, and four inches diameter.

I had at first selected β Canis minoris, as a proper star to be compared with α Aquilæ; but I found upon trial that it could not be seen in the day time, except under such favourable circumstances as could seldom be expected. I considered this at the time as a great disappointment; but I now find that the star (ι Pegasi) which I have substituted for it, is much better situated for the purpose. It has often been proposed to examine the parallax of a large star by comparing it with a smaller one; but to do this by meridian observations is very difficult, and in most cases impossible, from some peculiar practical difficulties which I am about to state.

For example: in the present case of α Aquilæ, the smaller star must have very nearly the same polar distance. If it follows the larger star within three hours of right ascension, it cannot be employed for this purpose, because in winter time it will pass the meridian before sun set; should the smaller star differ four hours or more in right ascension from the larger, it will in the summer pass the meridian after

sun rise, and be equally invisible. It is seldom that a star can be found within these very narrow limits; but relatively to α Aquilæ, it fortunately happens that γ Pegasi is so situated as to be very advantageously employed.

Not being yet perfectly satisfied with respect to the stability of the instrument, I have only computed those observations in which each star has been observed on the same day. In so short an interval as three hours, I cannot conceive any sensible change of position in the telescope can possibly affect the observations. Notwithstanding this precaution, they are far inferior in exactness to those of α Cygni. I can only attribute this to the effect of accidental refraction.

From the table * which accompanies this paper, it appears that 54 observations from the 25th July to 29th December, 1817, divided into four equal series, give the following results.

Difference in declination of α Aquilæ and γ Pegasi.

July 25, to Aug. 25,	-	-	1'49",17
Aug. 25, to Sept. 8,	-	-	1 49 ,20
Sept. 8, to Nov. 1,	-	-	1 49 ,61
Nov. 1, to Dec. 29,	-	-	1 48 ,57

If the first half be compared with the second half, the result will be,

July 25, to Sept. 8,	-	-	1'49",18
Sept. 8, to Dec. 29,	-	-	1 49 ,06

I can discern no appearance of parallax in the above observations; and indeed I have long considered it as a hopeless task to establish its existence by observations on a star so far from the zenith.

* Vide page 480.

After so many fruitless attempts to establish the existence of sensible parallax, I was much disposed to abandon all farther prosecution of this subject, when my anxiety was again renewed by the paper lately communicated to the Society by Dr. BRINKLEY. The arguments and observations which it contains, are such as no doubt require very attentive consideration ; but I think some of Dr. BRINKLEY's doubts have arisen from my not having myself been sufficiently explicit as to the details of my own observations, and the precautions I have used. However this may be, it seemed to me more than ever desirable to institute some new process of investigation, to which none of Dr. BRINKLEY's objections could possibly apply ; and it has occurred to me, that perhaps the observations made with the new transit instrument might be sufficiently exact for this purpose, though taken under very unfavourable circumstances. This was a question to be easily determined by inspection, and I have the satisfaction to state, that I find the observations of α *Aquilæ*, already made, quite sufficient to establish this important point ; namely, that the parallax of this star is either an insensible quantity, or is so extremely small, that it cannot possibly have had any share in producing the discordances observed by Dr. BRINKLEY.*

* These observations, as likewise some additional ones on other bright stars, continued to the month of September, will form the subject of another paper.

480 *Observations of α Aquilæ made with a fixed telescope.**

1817.	Observed Difference.	Difference re- duced to space.	Do. reduced to the beginning of 1817.	
	Rev. Parts.	' "	' "	
July 25	6.62	1.53.65	1 49.93	} 14 = 1.49,174
26	6.56	1.52.62	48.21	
27	6.63,5	1.53.90	49.61	
28	6.56,5	1.52.63	48.55	
30	6.53	1.52.11	48.11	
Aug. 1	6.64	1.54.00	49.40	
3	6.62	1.53.65	49.17	
5	6.63,5	1.53.90	49.39	
6	6.68	1.54.68	49.94	
9	6.65	1.54.16	49.44	
15	6.69	1.54.85	48.60	
17	6.68	1.55.05	49.58	
22	6.73	1.55.54	49.76	
25	6.65	1.54.16	48.75	} 13 = 1.49,203
31	6.78	1.56.40	49.65	
Sep. 3	6.61	1.53.48	47.28	
4	6.77	1.56.32	49.84	
5	6.66	1.54.30	47.35	
6	6.78	1.56.40	49.95	
7	6.63	1.53.82	47.71	
8	6.76	1.56.06	49.64	
10	6.80,5	1.56.82	50.66	
21	6.86	1.57.77	50.60	
24	6.74	1.55.71	48.41	
26	6.75	1.55.88	48.71	
27	6.80	1.56.74	50.45	} 14 = 1.49,613
28	6.82	1.57.08	49.40	
Oct. 1	6.81,5	1.56.99	50.15	
2	6.81,0	1.56.90	50.41	
3	6.84	1.57.42	49.74	
4	6.84	1.57.42	49.68	
faint 5	6.92	1.58.80	50.86	
6	6.86,5	1.57.85	49.96	
11	6.82	1.57.08	48.68	
17	6.88	1.58.11	49.90	
22	6.92	1.58.80	50.35	
28	6.90	1.58.46	49.70	
29	6.88	1.58.11	48.82	
30	6.84,5	1.57.50	48.30	} 13 = 1.48,568
31	6.92,5	1.58.89	49.25	
Nov. 1	6.88	1.58.11	48.78	
5	6.87,5	1.58.03	48.44	
11	6.93	1.58.97	49.08	
13	6.92	1.58.80	48.70	
15	6.91	1.58.62	48.08	
19	6.99,5	2. 0.10	49.37	
Dec. 7	7. 0	2. 0.18	48.62	
9	7. 2	2. 0.52	47.91	
11	7. 4,5	2. 0.95	48.84	
17	7.12	2. 1.70	49.19	
18	7. 2	2. 0.52	47.96	
26	7.11	2. 1.53	48.38	
27	7. 7	2. 0.85	47.88	
29	7.12	2. 1.70	48.93	

* The effect of parallax in the above observations should produce an apparent increase in the relative distance of the two stars. The extremely small difference, is in a contrary direction. No observation has been omitted in this result.

XXVI. *On the parallax of the fixed stars in right ascension.* By
John Pond, *F. R. S. Astronomer Royal.*

Read May 28, 1818.

IN the *Connoissance des Temps* for the year 1808, M. DELAMBRE, in a memoir on the parallax of the fixed stars, in which he relates the supposed discovery of a very considerable parallax, by two celebrated Italian astronomers,* proposes that this investigation should be attempted in right ascension.

I have often read this memoir with great attention, but have been prevented by accidental circumstances from carrying this suggestion into execution. The investigation has since that time become infinitely more difficult, from the narrow limits into which parallax has been restricted by more recent observations.

When the subject first engaged my attention, I was constantly in expectation of the new instrument, and was unwilling to engage in a series of observations which would so soon be interrupted; when this was erected, I was so much engaged in the other methods of investigation, that it was not possible to devote myself properly to this enquiry; it was only lately that it occurred to me to examine the observations as they stood on the transit book for other purposes; and I find that, notwithstanding they have not been regularly made at the two opposite seasons most favourable for detecting a parallax, yet a sufficient number of observations may be collected to establish the same conclusion as that which I have given in my former paper. The stars, of which I have a sufficient number of observations for this investiga-

tion, are α Aquilæ, Arcturus, Capella, and α Lyræ. Sirius is comparatively but seldom observed, from its vicinity to the horizon. As the observations of all the above stars give the same result, it will be sufficient to select one as an example. I have chosen α Aquilæ. The annexed table contains 120 observations of this star; and the following remarks will, I trust, be thought sufficiently conclusive to establish the point in question.

If a series of 12 results (obtained by 120 observations) be divided into two parts, first, according to the law of parallax, and next alternately, or in a manner perfectly accidental, then it so happens, that a greater difference is found in the latter or accidental mode of division, than in the former. From this it is evident, that the determination of the quantity of parallax is out of the power of the instrument to determine with this number of observations. The next question is, What are the limits within which parallax is restricted?

In examining several series of observations, I find that the result of 60 observations, reduced to the equator, and taken accidentally, never differs from the more correct mean derived from 120 by more than $0''.01$ of time, when reduced to the equator.

If of 12 results taken as above, the 6 least be classed together, and compared with the six greatest, the error of either class will not exceed the double of this quantity, or $0''.02$. It is therefore very highly improbable indeed, that an error of this magnitude should exist in a result deduced from 60 observations.

Since, therefore, the results arranged according to the law of parallax should differ by rather more than half the double parallax, I infer, that it is most highly improbable that the longer

axis of the ellipse described by parallax should, in the brightest stars, amount to $0''.5$ of space, and not probable that it should amount to half this quantity, or to $0''.25$. And when we consider that the minor axis of this ellipse is only measured in declination (and in α Aquilæ this is only equal to half the major axis); and that, moreover, the star is only deranged from its mean place, the half of this minor axis, I think it will not be very unsafe to conclude, that every attempt to discover the existence of parallax by a measure in declination, must end in disappointment.

These observations, continued for many years with the transit instrument, must in the end either detect the existence of parallax, or still more correctly define its limits. But these appear to me even now so small, that I am not disposed to institute any farther observations with a view to this particular subject, but shall leave it to be determined by the regular course of observation.*

* I take this opportunity of stating that the observations of α Cygni, continued in the manner described in a former paper, confirm, in the most decided manner, the total absence of any observable parallax. They are as follows :

	α Cygni.		β Aurigæ.		Difference of α and β Cygni.
	N ^o . of Obs.		N ^o . of Obs.	α Cygni & β Aurigæ	
				Sum	
Winter, 1817	25	8,173	28	9,818 = 17,984	5,226
Spring, —	26	7,920	29	10,044 = 17,964	
Summer, —	32	3,340	22	14,825 = 18,165	5,287
Autumn, —	25	4,075	17	14,067 = 18,142	
Winter, 1818	52	5,645	47	12,529 = 18,174	5,432

These observations seem to me to prove beyond a doubt that the parallax of α Cygni cannot much exceed one-tenth of a second of a degree. Vide Phil. Trans. 1817.

Observations of α Aquilæ reduced to Jan. 1, 1817

1816.	R Reduced.	1816.	R. Reduced.	1817.	R Reduced.
July 24	19 ^h 41' 48 ^s .07	Nov. 3	19 ^h 41' 48 ^s .10	Sept. 30	19 ^h 41' 51 ^s .06
25	48.09	8	48.03	Oct. 1	51.06
28	48.29	9	48.14	2	50.90
30	47.84	13	48.29	3	51.11
Aug. 3	48.09	15	48.21	6	51.16
4	48.17	17	48.20	7	51.10
8	48.06	18	48.14	8	51.26
15	48.09	22	47.98	11	51.12
18	48.06	24	48.18	17	51.06
21	48.21	29	48.09	Nov. 11	50.85
22	48.00	Dec. 30	48.04	13	51.07
25	48.09	7	48.21	20	51.00
26	47.86	8	48.14	22	51.02
27	48.14	1817. 20	48.09	28	51.09
28	48.18	Jan. 7	47.91	29	51.02
Sept. 2	48.09	8	48.14	9	50.96
3	48.20	Feb. 6	48.11	10	51.07
4	48.05	Mar. 4	48.25	11	51.12
5	48.09	6	48.23	15	51.10
10	48.06	9	48.23	17	50.88
11	48.13	10	50.94	22	51.02
12	48.24	21	51.09	24	51.09
14	48.14	22	51.09	26	50.95
15	48.20	30	51.18	27	51.00
16	47.92	July 28	51.10	1818. 29	51.07
22	48.07	Aug. 5	51.02	Jan. 16	51.12
26	48.07	6	51.12	17	50.80
27	48.26	12	51.02	20	51.07
30	48.23	15	51.11	24	50.21
Oct. 1	48.07	21	51.11	30	51.07
8	48.15	22	50.97	Feb. 13	53.82
16	48.05	29	50.98	14	54.08
22	48.17	Sept. 6	51.06	19	54.17
23	48.18	7	51.02	23	53.93
26	48.13	8	51.01	28	53.96
27	48.00	10	51.04	Mar. 1	53.91
28	48.12	19	51.00	2	53.94
29	48.01	21	51.15	3	54.00
31	48.09	27	51.11	5	54.16
Nov. 1	48.13	29	51.03	7	54.00

Result of 120 Observations.

1816. July 24 to Aug. 21	-	51".020
Aug. 22 to Sept. 10	-	.020
Sept. 11 to Oct. 1	- -	.063
Oct. 8 to Nov. 1	-	.033
Nov. 3 to Nov. 29	-	.066
Nov. 30 to Mar. 9 (1817)		.065
1817. Mar. 10 to Aug. 21	-	.078
Aug. 22 to Sept. 29	-	.037
Sept. 30 to Nov. 11	-	.068
Nov. 13 to Dec. 17	-	.033
Dec. 22 to Jan. 30 (1818)		.040
1818. Feb. 13 to Mar. 7	-	.067
Mean	-	51".049

Results according to the law of parallax.

	Neutral.	Minimum.
	51".020	51".020
	.065	.063
	.078	.033
	.033	.066
	.040	.037
	.067	.068
Mean	51".050	51".048
	51".049	51".049
Error	= .001	Error = .001

Taken alternately or accidentally.

	51".020	51".020
	.063	.033
	.066	.065
	.078	.037
	.068	.033
	.040	.067
Mean	51".056	51".043
	51".049	51".049
Error	= 0.007	Error = .006

Six least. Six greatest.

	51".020	51".063
	.020	.066
	.033	.065
	.033	.078
	.037	.068
	.040	.067
Mean	51".030	51".068
	51".049	51".049
Error	= .019	Error = .019

XXVII. *An abstract of the results deduced from the measurement of an arc on the meridian, extending from latitude $8^{\circ} 9' 38''$,₄, to latitude $18^{\circ} 3' 23''$,₆, N. being an amplitude of $9^{\circ} 53' 45''$,₂. By Lieut. Colonel William Lambton, F. R. S. 33d Regiment of foot.*

Read May 21, 1818.

IN the 12th vol. of the Asiatick Researches, there are detailed accounts of two complete sections of an arc on the meridian, measured by me at different times, in prosecuting the trigonometrical survey of the Peninsula of India. The first is comprehended between the parallels of Punnaë, a station near Cape Comorin, in latitude $8^{\circ} 9' 38''$,₃₉, and Patchipolliam in Coimbetoor, in latitude $10^{\circ} 59' 48''$,₉₃; being an amplitude of $2^{\circ} 50' 10''$,₅₄. The second is comprehended between the parallels of Patchipolliam and Namthabad, a station near Gooty, in the ceded districts; and lying in latitude $15^{\circ} 6' 0''$,₂₁, gives an amplitude of $4^{\circ} 6' 11''$,₂₈. Since these measurements were made, I have had the good fortune to get another section, extending from Namthabad to Daumergidda, in the Nizam's dominions, which being in latitude $18^{\circ} 3' 23''$,₆, gives an increase of $2^{\circ} 57' 23''$,₃₂; making in the whole an arc of $9^{\circ} 53' 45''$,₁₄ in amplitude; the longest *single* arc that has ever been measured on the surface of this globe. The detailed accounts of this last arc, with various conclusions concerning the three sections, have been presented to the Asiatick Society by the Marquess of HASTINGS, the present Governor General, and will be published in the 13th volume

of the Asiatick Researches. But as it may be three or four years before that volume makes its appearance, I have been induced to draw out this abstract of the results, thinking that the conclusions herein contained may be interesting to the Royal Society, and to the astronomers of Europe. As a reference may be made to the volumes above mentioned, I have simply given the lengths of the sides of the triangles from which the arcs are deduced, together with the lengths of the terrestrial and celestial arcs, without including either the tables of triangles, or the particulars of the astronomical observations, farther than the names of the stars, and the mean lengths of the celestial arcs, and, consequently, the mean degrees deduced therefrom.

The first of these sections gives the degree due to latitude $9^{\circ} 34' 44''$, the middle point of that arc, equal 60472,83 fathoms. The second section, whose middle point is in latitude $13^{\circ} 2' 55''$, gives the mean degree equal 60487,56 fathoms. The last section gives the degree equal 60512,78 fathoms, due to the latitude of $16^{\circ} 34' 42''$, the middle point of that section.

In my second paper, in the 12th vol. of the Asiatick Researches, it appeared that the degree due to latitude $11^{\circ} 37' 49''$, the middle point between Punnae and Namthabad, was 60480,3 fathoms. Since that paper was sent, there has been a small correction applied to the base near Gooty, after comparing the chains with the brass standard scale. This correction has somewhat increased the meridional distance between that base and Yerracondah south; and, consequently, the whole terrestrial arc between Namthabad and Punnae is also increased, which now gives the degree due to latitude

$11^{\circ} 37' 49''$, equal 60481.55 fathoms. However, as there are now three distinct sections, whose respective middle points lie in $9^{\circ} 34' 44''$; $13^{\circ} 2' 55''$; and $16^{\circ} 34' 42''$; I have thought it best to take the degrees due to these latitudes, as deduced from actual observation, using each, *first* with the French measure, *then* with the English, and *lastly* with the Swedish measure; and thence obtaining a general mean for the compression at the poles. The *first mean* of these three degrees used with the French degree, gives the compression $\frac{1}{309.15}$. The *second mean* of the same three degrees used with the English degree, gives $\frac{1}{313.54}$. And the *third mean* of these three degrees used with the Swedish degree, gives $\frac{1}{307.19}$ for the compression; so that the mean of these three means will give the compression at the poles $\frac{1}{309.96}$, or $\frac{1}{310}$ nearly of the polar axes; and this has been finally adopted for computing the general tables of degrees from the equator to the pole.

It will be seen by inspecting the plan of the triangles, (Pl. XXIII.) that all the sides from which the arc has been deduced lie so near the meridian, that no correction has been required; a circumstance that has saved much trouble. The sides being so nearly north and south, that the base reduced from each side as an hypotenuse, may be considered as a chord of an arc parallel, and so nearly contiguous to the meridian, that it may, as to sense, be taken as the chord to the same arc on the meridian; and these chords being in general short, they will be the same as the arcs, very nearly. I have therefore not been at the trouble of applying any correction; for if the whole arc between Punnae and Daumergidda (upwards of 680 miles) were divided into small arcs of 30

miles each, the whole difference between these arcs and their chords would not be more than a fathom and a half.

The number of base lines in this extensive arc, are five; all measured with the chain extended in coffers; with elevating screws, &c; and every part of the operation has been performed with the greatest possible attention. The one near Bangalore may be considered as the first; and its height above the sea was obtained by a series of triangles connecting it with another base near St. Thomas's Mount, whose height above the low water mark was determined by observations made at the sea beach, and at the race stand near the north end of the base (*Asiat. Res. vol.viii.*). The base lines to the southward are, the one in Coimbetoor, and the other near Tinnivelly, whose heights above the sea were determined from the Bangalore base. Those to the northward are, the base near Gooty, and the one near Daumergidda. The account of this last measurement, and of the curious experiments for comparing the steel chains with the brass standard scale, will appear in the 13th volume of the *Asiatick Researches*. The particulars of the other measurements may be seen in the 10th and 12th volumes of the same work.

The great station of observation at Doddaguntah, is near the first base line; and it was at that station where the position of the meridian was fixed for extending it to the north and south. The latitude of that station was also determined by observing the zenith distances of a number of stars from the Greenwich catalogue for 1803. That latitude, however, was afterwards set aside from a supposed disturbance of the plummet. The latitude was afterwards fixed from observations made at Punnaë, the southernmost station of

observation, by setting off degrees corresponding to different latitudes, after they were finally determined.

As Doddaguntah station has been left out in the divisions of the grand arc, its latitude was only useful in fixing that of Savendroog, one of the great meridian stations for crossing the Peninsula (*Asiatick Researches*, vol. i.). The other remaining stations of observation are, Patchipolliam, or Coim-betoor; Namthabad, near Gooty; and Daumergidda, in the Nizam's dominions. We shall now see how these stations are connected by the triangles; Doddaguntah being the referring station in counting the northings, southings, &c.; and to whose meridian the whole terrestrial arc is reduced.

Without regarding the order of time, we will set off from Doddaguntah to Patchipolliam, and thence to Punnaë. After which we will return to Doddaguntah, and proceed northerly to Namthabad, and from Namthabad to Daumergidda.

TABLE I. Lengths of the terrestrial arc comprehended between the parallels of Doddaguntah station, and the station near Patchipolliam.

Stations at	Stations to	Bearings	Distances	Distances on the		Distance from Doddaguntah	
				Perpendicular	Meridian	Perpendicular	Meridian
Doddaguntah	Deorbetta	0° 13' 08" 43 SE	135931,3	519,6 E	135930,3 S	519,6 E	135930,3 S
Deorbetta	Ponnasmalli	2 11 54,36 SE	174071,7	6677,6 E	173947,6 S	7197,2 E	309877,9 S
Ponnasmalli	Woorachmalli	3 49 54,39 SE	243502,4	16272,6 E	242958,2 S	23469,8 E	552836,1 S
Woorachmalli	Patchipolliam	7 53 51,52 SW	270169,4	24206,4 W	174498,5 S	736,6 W	7273346,6 S

TABLE II. Length of the terrestrial arc comprehended between the parallels of Doddaguntah station and the station at Namthabad.

Stations at	Stations to	Bearings	Distances	Distances on the		Distance from Doddaguntah	
				Perpendicular	Meridian	Perpendicular	Meridian
Deorbetta	Allasoor hill	0° 43' 54",55 NW	194662,8	2486,3 W	194646,9 N	1966,7 W	58716,6 N
Allasoor hill	Kulcottah hill	4 5 43,25 NW	94211,8	6728,3 W	93971,2 N	8695,0 W	152687,8 N
Kulcottah hill	Yerracondah	5 43 49,55 NE	18088,8	18060,9 E	179979,9 N	9365,9 E	332667,7 N
Yerracondah	Ooracondah	7 04 21,49 NW	126785,7	15610,8 W	125821,0 N	6244,9 W	438488,7 N
Ooracondah	Davurcondah	5 32 52,09 NE	150506,1	14550,4 E	149801,1 N	8305,5 E	628289,8 N
Davurcondah	Gooty Droog	0 16 40,56 NE	158946,2	771,1 E	158944,3 N	9070,6 E	767234,1 N
Gooty Droog	Namthabad	70 43 30,60 SW	16472,2	15548,9 W	5437,5 S	6472,3 W	761796,6 N

TABLE III. *Length of the terrestrial arc between the parallels of Patchipolliam and the station near Punne.*

Stations at	Stations to	Bearings	Distances or the		Distance from Doddaguntah	
			Perpendicular	Meridian	Perpendicular	Meridian
Patchipolliam	Partemalli	7° 56' 29.74 SW	16656.1 W	119397.4 S	16656.1 W	119397.4 S
Partemalli	Peermaultmali	1 54 37.23 SW	4444.2 W	133247.8 S	21100.3 W	252642.2 S
Peermaultmali	Sudraghetry	11 14 52.96 SE	40392.4 E	203102.5 S	19212.1 E	455747.7 S
Sudraghetry	Vulunkota	3 13 50.25 SW	19127.1 W	338804.1 S	165.0 E	794608.8 S
Vulunkota	Kunnamopolha	0 10 35.28 SW	333.8 W	103903.3 S	168.8 W	922977.1 S
Kunnamopolha	Punne station	0 27 21.16 SE	1003.6 E	125128.4 S	834.8 E	1029100.5 S

TABLE IV. *Length of the terrestrial arc comprehended between the parallels of Doddaguntah and Daumergidda.*

Stations at	Stations to	Bearings	Distances on the		Distance from Doddaguntah	
			Perpendicular	Meridian	Perpendicular	Meridian
Gooty Droog	Koelcondah	10° 59' 34.9 NW	15035.6 W	73342.5 N	5359.0 W	842576.5 N
Koelcondah	Poolycondah	4 06 33.9 NW	535.7 W	53045.0 N	9434.7 W	595521.6 N
Poolycondah	Kerrabellagal	13 21 56.9 NE	29800.9 E	124455.5 N	20136.2 E	1020976.9 N
Kerrabellagal	Daroor hill	3 04 35.9 NW	2038.4 W	150234.1 N	12447.8 E	1171451.0 N
Daroor hill	Impahgut	0 45 15.7 NW	2320.1 W	175144.0 N	9741.7 E	1346605.0 N
Impahgut	Kotakodangul	1 42 38.8 NW	3523.5 W	153794.7 N	5148.2 E	1500399.7 N
Kotakodangul	Shelapilly hill	2 23 25.7 NE	9444.9 E	231574.5 N	14613.1 E	1731974.2 N
Shelapilly hill	Daumergidda	0 01 33.0 NE	45.6 E	102505.6 N	14659.7 E	1335224.8 N

From the foregoing tables we collect the following particulars.

	Terrestrial arcs.	
	feet	fathoms
By Table I. the terrestrial arc between Doddaguntah and Patchipolliam is	727334,6	
By Table II. The arc between Doddaguntah and Namthabad is	761796,6	
Their sum will be the arc between Patchipolliam and Namthabad =	1489131,2	248188,53
By Table III. the arc between Patchipolliam and Punnæ station is	1029100,5	171516,75
Their sum is the terrestrial arc between Punnæ and Namthabad =	2518231,7	
By Table IV. the arc between Doddaguntah and Daumergidda is	1835224,8	
From which subtract the arc between Doddaguntah and Namthabad	761796,6	
The difference will be the arc between Namthabad and Daumergidda =	1073428,2	178904,70
And the sum of the whole of these three sections will be the terrestrial arc between Punnæ and Daumergidda	-	598609,98

Amplitudes of the celestial arc between the parallels of Punnæ and Patchipolliam.

Stars.	Zenith distances at		Amplitudes.
	Patchipolliam.	Punnæ.	
♂ Hydræ	4° 37' 12",65 S	1° 47' 01",37 S	2° 50' 11",28
♂ Hydræ	3 52 08,97 S	1 01 59,31 S	9,66
♂ Cancrī	1 36 32,64 N	4 26 42,91 N	10,27
♂ Leonis	0 13 18,16 S	2 36 52,07 N	10,23
Regulus	1 55 12,99 N	4 45 24,06 N	11,07
♂ Leonis	5 29 54,26 N	8 20 03,44 N	9,18
♂ Leonis	4 39 59,40 N	7 30 11,59 N	12,19
♂ Virginis	1 00 55,20 N	3 51 05,95 N	10,75
♂ Serpentis	0 12 14,15 N	3 02 25,36 N	11,21
♂ Serpentis	3 56 48,46 S	1 06 38,10 S	10,36
♂ Herculis	3 37 38,58 N	6 27 48,35 N	9,77
♂ Ophiuchi	1 43 00,69 N	4 33 11,86 N	11,17
♂ Aquilæ	2 35 16,44 N	5 25 29,25 N	12,81
♂ Aquilæ	0 50 50,74 S	1 59 19,11 N	10,51
♂ Atair	2 37 54,13 S	0 12 14,69 N	8,82
♂ Aquilæ	5 03 55,68 S	2 13 48,40 S	7,28
♂ Delphini	2 55 48,68 N	5 45 58,28 N	12,60
Mean			2 50 10,55

*Amplitude of the celestial arc between the parallels of Patchipolliam
and Namthabad.*

Stars.	Zenith distances at		Amplitudes.
	Patchipolliam.	Namthabad.	
◦ Leonis	0° 13' 18", 16 S	4° 19' 29", 91 S	4° 06' 11", 75
Regulus	1 55 12, 99 N	2 10 59, 16 S	12, 15
♂ Leonis	5 29 54, 26 N	1 23 42, 08 N	12, 18
♂ Leonis	4 39 59, 40 N	0 33 49, 17 N	10, 23
♂ Virginis	1 00 55, 20 N	3 58 56, 58 S	10, 07
♂ Serpēntis	0 12 14, 15 N	3 53 56, 58 S	10, 23
♂ Herculis	3 37 38, 58 N	0 28 34, 09 S	12, 67
♂ Ophiuchi	1 43 00, 69 N	2 23 10, 99 S	11, 68
♂ Aquilæ	2 35 16, 44 N	1 30 53, 62 S	10, 06
♂ Aquilæ	0 50 50, 74 S	4 57 02, 59 S	11, 80
Atair	2 37 54, 13 S	6 44 07, 19 S	13, 06
♂ Delphini	2 55 45, 68 N	1 10 23, 40 S	9, 08
Mean			4 06 11, 28

*Amplitude of the celestial arc between the parallels of Namthabad
and Daumergidda.*

Stars.	Zenith distances at		Amplitudes.
	Namthabad.	Daumergidda.	
◦ Leonis	4° 19' 30", 079 S	7° 16' 53", 233 S	2° 57' 23", 254
Regulus	2 10 59, 463 S	5 08 21, 582 S	22, 099
♂ Leonis	5 43 28, 704 S	2 46 03, 056 N	25, 648
♂ Leonis	1 23 42, 038 N	1 33 40, 925 S	22, 963
♂ Leonis	0 33 49, 174 N	2 23 34, 324 S	23, 498
♂ Virginis	3 05 14, 750 S	6 02 36, 570 S	21, 820
♂ Bootis	4 16 54, 460 N	1 19 29, 335 N	25, 125
Arcturus	5 06 16, 777 N	2 08 51, 502 N	25, 272
♂ Bootis	0 31 35, 868 S	3 28 55, 050 S	19, 182
♂ Serpēntis	3 53 56, 290 S	6 51 19, 536 S	23, 246
♂ Serpēntis	0 56 32, 646 N	2 00 50, 287 S	22, 933
♂ Serpēntis	1 12 23, 718 N	1 45 00, 865 S	24, 583
♂ Herculis	4 31 19, 103 N	1 33 55, 469 N	23, 634
Mean			2 57 23, 320

*Amplitude of the whole celestial arc between the parallels of
Punnæ and Daumergidda.*

Stars.	Zenith distances at		Amplitudes.
	Punnæ.	Daumergidda.	
• Leonis	2° 36' 51", 926 N	7° 16' 53", 233 S	9° 53' 45", 159
Regulus	4 45 23 , 979 N	5 08 21 , 582 S	45 , 561
θ Leonis	8 20 03 , 213 N	1 33 40 , 925 S	44 , 138
β Leonis	7 30 11 , 608 N	2 23 34 , 324 S	45 , 932
• Virginis	3 51 06 , 083 N	6 02 36 , 570 S	42 , 653
δ Serpentis	3 02 25 , 643 N	6 51 19 , 536 S	45 , 179
γ Serpentis	8 08 47 , 269 N	1 45 00 , 865 S	48 , 134
Mean			9 53 45 , 251

*Latitude of Punnæ station deduced from the foregoing zenith
distances of eight principal stars, whose declinations and annual
variations are given in the Greenwich observations for 1802.*

Stars.	For the beginning of 1805.		Latitudes.
	Mean declination.	Correct. zen. dist.	
Regulus	12° 54' 58", 93 N	4° 45' 24", 09 N	8° 09' 34", 84 N
β Leonis	15 39 45 , 28	7 30 11 , 59 N	33 , 70
α Serpentis	7 03 00 , 30	1 06 38 , 10 S	38 , 40
α Herculis	14 37 30 , 96	6 27 48 , 35 N	42 , 61
α Ophiuchi	12 42 50 , 91	4 33 11 , 86 N	39 , 05
γ Aquilæ	10 08 58 , 34	1 59 19 , 77 N	38 , 57
Atair	8 21 53 , 53	0 12 14 , 69 N	38 , 84
β Aquilæ	5 55 52 , 71	2 13 48 , 40 S	41 , 11
Mean			8 09 38 , 39

Latitude of Punnæ station - - - 8° 09' 38", 39

Celestial arc between Punnæ and Patchipolliam 2 50 10 , 54

Their sum is the latitude of Patchipolliam 11 59 48 , 93

Celestial arc between Patchipolliam and Nam-

thabad	-	-	-	-	-	<u>4° 06' 11",28</u>
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Which added to the last gives the latitude of

Namthabad	-	-	-	-	-	15 06 00 ,21
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Celestial arc between Namthabad and Daumer-

gidda	-	-	-	-	-	2 57 23 ,32
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Which added gives the latitude of Daumer-

gidda	-	-	-	-	-	18 03 23 ,53
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The arc between Punnæ and Daumergidda by

seven corresponding stars ($9^{\circ} 53' 45'',25$)

added to the latitude of Punnæ ($8^{\circ} 09' 38'',$

39) gives	-	-	-	-	-	<u>18 03 23 ,64</u>
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Mean of these two latitudes gives the correct

latitude	-	-	-	-	-	18 03 23 ,58
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By comparing the above three sections of the celestial arc with their respective terrestrial measures, we shall have the following conclusions.

Celestial arc between Punnæ and Patchipolliam	2° 50' 10'',54
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Latitude of the middle point ($9^{\circ} 34' 43'',6$)	9 34 44
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Terrestrial arc in fathoms	-	+	171516 ,75
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Mean length of the degree due to latitude $9^{\circ} 34'$

44" in fathoms	-	-	-	60472 ,83
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Celestial arc between Patchipolliam and Nam-

thabad	-	-	-	-	4° 06' 11",28
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Latitude of the middle point	-	-	13 02 55
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Terrestrial arc in fathoms	-	-	248188 ,53
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Mean degree due to latitude $13^{\circ} 2' 55''$	-	60487 ,56
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Celestial arc between Namthabad and Daumer-

gidda	-	-	-	-	2° 57' 23'',32
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Terrestrial arc in fathoms - - - 178904,70

Latitude of the middle point $16^{\circ} 34' 42''$

Mean degree in fathoms due to latitude $16^{\circ} 34' 42''$ 60512,78

So that by the above comparisons it appears that the degree due to latitude - $9^{\circ} 34' 44''$ is - 60472,83

the degree due to latitude $12^{\circ} 2' 55''$ is - 60487,56

the degree due to latitude $16^{\circ} 34' 42''$ is - 60512,78

It now remains to compare each of these mean degrees, *first* with the French measurement; *then* with the English; and *lastly* with the Swedish; and by proceeding on the elliptic theory, deduce from these data three mean ellipticities, and from these three a *general mean*, which must give nearly the true compression at the poles.

Previous to this determination, it will be necessary to investigate the requisite formulæ for obtaining the compression by a comparison of measured degrees in distant latitudes; and first, by the measured degrees on the meridian.

Let m' and m be the measured degrees, in latitudes l' and l ; and let a represent the equatorial diameter, and b the polar axis: that is, supposing the earth to be an ellipsoid, let a and b represent the transverse and conjugate axes of an elliptic meridian. Then it is known from conic sections and the nature of curvature, that $\frac{a^2 b^2}{2 (\cos.^2 l'. a^2 + \sin.^2 l'. b^2)^{\frac{3}{2}}}$ is the radius of curvature of the ellipse at l' ; and that $\frac{a^2 b^2}{2 (\cos.^2 l. a^2 + \sin.^2 l. b^2)^{\frac{3}{2}}}$ is the radius of curvature of the same, or any other elliptic meridian, on the same ellipsoid, at the point l . And since the degrees at l' and l are as the radii of curvature at these points, we have $m': m :: \frac{a^2 b^2}{2 (\cos.^2 l'. a^2 + \sin.^2 l'. b^2)^{\frac{3}{2}}} : \frac{a^2 b^2}{2 (\cos.^2 l. a^2 + \sin.^2 l. b^2)^{\frac{3}{2}}}$

$$:: (\text{Cos.}^2 l' a^2 + \text{Sin.}^2 l' b^2) - \frac{1}{2} : (\text{Cos.}^2 l a^2 + \text{Sin.}^2 l b^2) - \frac{1}{2}.$$

Now to simplify this expression, if $a = 1$, e the ellipticity; and therefore $b = 1 - e$, and $b^2 = 1 - 2e$ nearly, because e^2 must be very small. Then will $m' : m :: (\text{Cos.}^2 l' + (1 - 2e) \text{Sin.}^2 l') - \frac{1}{2} : (\text{Cos.}^2 l + (1 - 2e) \text{Sin.}^2 l) - \frac{1}{2}$. And if $(1 - \text{Sin.}^2 l')$ and $(1 - \text{Sin.}^2 l)$ be substituted for $\text{Cos.}^2 l'$ and $\text{Cos.}^2 l$, the expression will be transformed into $(1 - 2e \text{Sin.}^2 l') - \frac{1}{2}$ and $(1 - 2e \text{Sin.}^2 l) - \frac{1}{2}$; or $1 + 3e \text{Sin.}^2 l'$ and $1 + 3e \text{Sin.}^2 l$ nearly, by developing the series, and leaving out all the quantities involving e^2 or its higher powers. Hence $m' : m :: 1 + 3e \text{Sin.}^2 l' : 1 + 3e \text{Sin.}^2 l$. (1)

$$\text{which reduced gives } e = \frac{m' - m}{3(m \text{Sin.}^2 l' - m' \text{Sin.}^2 l)} \quad (2)$$

and when the degrees are contiguous, or very near to each other, this expression may be rendered still more simple by making $m' = m$ in the denominator, which under these circumstances will scarcely affect the result: whence

$$e = \frac{m' - m}{3m (\text{Sin.}^2 l' - \text{Sin.}^2 l)} \quad (3)$$

By this expression it will be easy to estimate the increments to degrees lying contiguous to each other: for if m , m' , m'' , &c. be contiguous degrees in latitudes l ; l' , l'' , &c. that is l , $l + 1^\circ$; $l + 2^\circ$ &c. Then we shall have

$$e = \frac{m'' - m}{3(\text{Sin.}^2 l'' - \text{Sin.}^2 l)}; \text{ which being made equal to } \frac{m' - m}{3(\text{Sin.}^2 l' - \text{Sin.}^2 l)}$$

and reduced, we get $m' - m : m'' - m :: \text{Sin.}^2 l' - \text{Sin.}^2 l : \text{Sin.}^2 l'' - \text{Sin.}^2 l$; and in like manner $m'' - m : m''' - m :: \text{Sin.}^2 l'' - \text{Sin.}^2 l : \text{Sin.}^2 l''' - \text{Sin.}^2 l$; and $m''' - m : m'''' - m :: \text{Sin.}^2 l''' - \text{Sin.}^2 l : \text{Sin.}^2 l'''' - \text{Sin.}^2 l$. &c. from which it appears that the increments to the degrees, beginning with the lowest latitude, will always be as the increments to the

squares of the sines of the corresponding latitudes: and if m be at the equator where the $\text{Sin. } l$ is 0, then we shall have $m' - m : m'' - m :: \text{Sin.}^2 l' : \text{Sin.}^2 l$.

Since by equation 1, $m' : m :: 1 + 3e. \text{Sin.}^2 l$:

$$1 + 3e. \text{Sin.}^2 l; \text{ then } m' = m \left(\frac{1 + 3e. \text{Sin.}^2 l}{1 + 3e. \text{Sin.}^2 l} \right) \dots (4)$$

$$\text{and } m = m' \left(\frac{1 + 3e. \text{Sin.}^2 l}{1 + 3e. \text{Sin.}^2 l} \right) \dots (5)$$

When m is at the equator, and therefore $\text{Sin. } l = 0$; then

$$m' = m (1 + 3e. \text{Sin.}^2 l) \dots (6)$$

If m' be at the pole, and therefore $\text{Sin.}^2 l = 1$, then we

$$\text{have } m' = m \left(\frac{1 + 3e}{1 + 3e. \text{Sin.}^2 l} \right) \dots (7)$$

If the degrees perpendicular to the meridian be made use of, let p' and p be the measures of those degrees in latitudes l' and l , then the radius of curvature of the perpendicular degree at l' being as $\frac{1}{2(1 - 2e. \text{Sin.}^2 l')^{\frac{1}{2}}} = \frac{1}{2} (1 - 2e. \text{Sin.}^2 l')^{\frac{1}{2}} = \frac{1}{2} (1 + e. \text{Sin.}^2 l')$ very nearly; and for the same reason the radius of curvature of the perpendicular degree at l will be as $\frac{1}{2} (1 + e. \text{Sin.}^2 l)$ very nearly; so that we get $p' : p :: 1 + e. \text{Sin.}^2 l' : 1 + e. \text{Sin.}^2 l$ (8)

$$\text{and when reduced gives } e = \frac{p' - p}{p. \text{Sin.}^2 l' - p' \text{Sin.}^2 l} \dots (9)$$

$$\text{From equation 8, } p' = p \left(\frac{1 + e. \text{Sin.}^2 l'}{1 + e. \text{Sin.}^2 l} \right) \dots (10)$$

$$\text{and } p = p' \left(\frac{1 + e. \text{Sin.}^2 l}{1 + e. \text{Sin.}^2 l'} \right) \dots (11)$$

If p be on the equator where the $\text{Sin. } l$ vanishes, then equation 10 becomes $p' = p (1 + e. \text{Sin.}^2 l')$ (12)

If p' be at the pole, and therefore the cosine of l' unity, then equation 10 becomes $p' = p \left(\frac{1 + e}{1 + e. \text{Sin.}^2 l} \right)$ (13)

Since the degree on the meridian, and the degree perpendicular to the meridian, are equal at the pole, we shall have by equations 7, and 13, $p \left(\frac{1+e}{1+e \sin^2 l} \right) = m \left(\frac{1+3e}{1+3e \sin^2 l} \right)$, where p and m are in the same latitude l . Now $m \left(\frac{1+3e}{1+3e \sin^2 l} \right) = m (1+3e) \cdot (1+3e \sin^2 l)^{-1} = m (1+3e \cos^2 l)$ nearly; and therefore $p \left(\frac{1+e}{1+e \sin^2 l} \right) = p(1+e) \cdot (1+e \sin^2 l)^{-1} = p(1+e \cos^2 l)$ nearly. Hence $m(1+3e \sin^2 l) = p(1+e \cos^2 l)$ which reduced gives $e = \frac{p-m}{(3m-p) \cos^2 l} \dots \dots \dots (14)$

Since $m(1+3e \cos^2 l) = p(1+e \cos^2 l)$, we get

$$m : p :: 1 + e \cos^2 l : 1 + 3e \cos^2 l \dots \dots (15)$$

and when $l = 0$, and its Cos. equal 1, then $m : p :: 1 + e : 1 + 3e \dots \dots \dots (16)$

If we make use of the degrees of longitude, then let d' and d represent their measures in latitudes l' and l ; and their respective radii of curvature at l' and l will be expressed by

$$\frac{\cos. l'}{2 (\cos^2 l' + (1-2e) \sin^2 l')^{\frac{3}{2}}} \text{ and } \frac{\cos. l}{2 (\cos^2 l + (1-2e) \sin^2 l)^{\frac{3}{2}}}, \text{ and therefore } d' : d :: \frac{\cos. l'}{(\cos^2 l' + (1-2e) \sin^2 l')^{\frac{3}{2}}} : \frac{\cos. l}{(\cos^2 l + (1-2e) \sin^2 l)^{\frac{3}{2}}};$$

$$\text{that is } d' : d :: \frac{\cos. l'}{(1-2e \sin^2 l')^{\frac{3}{2}}} : \frac{\cos. l}{(1-2e \sin^2 l)^{\frac{3}{2}}}; \text{ that is } d' : d ::$$

$$\cos. l' (1-2e \sin^2 l')^{-\frac{3}{2}} : \cos. l (1-2e \sin^2 l)^{-\frac{3}{2}}; \text{ that is } d' : d :: \cos. l' (1+e \sin^2 l') : \cos. l (1+e \sin^2 l) \dots \dots (17)$$

$$\text{and this reduced gives } e = \frac{d' \cos. l - d \cos. l'}{d \cos. l' \sin^2 l - d' \cos. l \sin^2 l} \dots \dots (18)$$

since $d' : d :: \cos. l' (1+e \sin^2 l') : \cos. l (1+e \sin^2 l)$; if d be at the equator where $\sin. l$ vanishes; then

$$d : d' :: 1 : \cos. l' (1+e \sin^2 l') \dots \dots \dots (19)$$

From this equation we get $d = \frac{d'}{\cos. 'l (1 + e. \sin.^2 'l)}$.

And from equation 9 we get $p = \frac{p'}{1 + e. \sin.^2 'l}$; and since at the equator the degree of longitude, and the perpendicular degree are equal, then $\frac{d'}{\cos. 'l (1 + e. \sin.^2 'l)} = \frac{p'}{1 + e. \sin.^2 'l}$, and this reduced we shall have $d' = p'. \cos. 'l$, where d' and p' are in the same latitude. Hence $d': p': \cos. 'l: \text{rad} \dots (20)$

I shall now proceed to determine the compression at the poles from the foregoing three sections of the arc; and comparing each, *first* with the French degree in latitude $47^{\circ} 30' 46''$, equal 60779 fathoms; *then* with the English degree in latitude $52^{\circ} 2' 20''$, which is 60820 fathoms; and *lastly* with the Swedish degree in latitude $66^{\circ} 20' 12''$ when it is 60955 fathoms.

With respect to the French degree, as there appears much irregularity in the different sections of the arc between Dunkirk and Montjouy, I have used *that* between the Pantheon at Paris, and Eveaux, as given by DE LAMBRE in the *Base du Systeme Métrique*, as it appears to be the most consistent. This degree is 57066 toises, equal to 60798 fathoms. But their measurements being all reduced to the temperature of 32° of FAHRENHEIT'S thermometer, the above degree will require a deduction of 19 fathoms nearly, to make it what it would have measured by the brass standard at the temperature of 62° , which is our standard temperature. Hence, 60779 fathoms is the degree in latitude $47^{\circ} 30' 46''$ to compare with the Indian measurements.

Let this degree be denoted by m' ; and its latitude, $47^{\circ} 30' 46''$ by $'l$; and let the degree 60472.83 fathoms be m , and its latitude $9^{\circ} 34' 44''$ be l . Then by equation 1,

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$e = \frac{m' - m}{3(m \cdot \text{Sin.}^2 l - m' \cdot \text{Sin.}^2 l')}$. We shall then have the type calculation as follows :

$$\begin{array}{rcl} l & = & 9^\circ 34' 44'' \\ l' & = & 47 \ 30 \ 46 \end{array} \qquad \begin{array}{rcl} m & = & 60472.83 \\ m' & = & 60779 \\ \hline m' - m & = & 306.17 \end{array}$$

$$\begin{array}{rcl} \text{Log. } m & = & 4.7815603 \\ \text{log. (Sin.}^2 l) & = & 1.7354392 \end{array}$$

$$\hline 4.5169996. \text{ its nat. no. } = 32885.1$$

$$\begin{array}{rcl} \text{Log. } m' & = & 4.7837536 \\ \text{log. (Sin.}^2 l') & = & 2.4423288 \end{array}$$

$$\hline 3.2260824. \text{ nat no. } .1683.0$$

$$\hline 31202.1 = (m \text{ S.}^2 l - m' \text{ S.}^2 l') \\ 3$$

$$\text{Hence } e = \frac{306.17}{93606.3} = \frac{1}{305.73} \frac{1}{93606.3}$$

$$\begin{array}{rcl} \text{Let } l & = & 19^\circ \ 2' \ 55'' \ \therefore m = 60487.56 \\ l' & = & 47 \ 30 \ 46 \ \therefore m' = 60779 \end{array}$$

$$\hline 291.44 = m' - m$$

$$\begin{array}{rcl} \text{Log. } m & = & 4.7816660 \\ \text{log. (Sin.}^2 l) & = & 1.7354392 \end{array}$$

$$\hline 4.5171052 \therefore \text{ nat. no. } = 32893.2$$

$$\begin{array}{rcl} \text{Log. } m' & = & 4.7837536 \\ \text{log. (Sin.}^2 l') & = & 2.7073618 \end{array}$$

$$\hline 3.4911154 \therefore \text{ nat. no. } = 3098.3$$

$$\text{Hence } e = \frac{291.44}{89384.7} = \frac{1}{306.70}$$

$$\hline 29794.9 \\ 3$$

from the measurement of an arc of the meridian, &c. 503

$$\text{Let } l = 16^\circ 34' 42'' \dots m = 60512,78$$

$$l' = 47^\circ 30' 46'' \dots m' = 60779 \dots$$

$$m' - m = 266,22$$

$$\text{Log. } m = 4,7818471$$

$$\text{log. (Sin.}^2 l) = 1,7354392$$

$$4,5172463 \dots \text{nat. no.} = 32903,8$$

$$\text{Log. } m' = 4,7837536$$

$$\text{log. (Sin.}^2 l') = 2,9106824$$

$$3,6944360 \dots \text{nat. no.} \dots 4948,1$$

$$27955,7$$

$$8$$

$$\text{Hence } e = \frac{266,22}{83867,1} = \frac{1}{315,03}$$

$$83867,1$$

Whence the mean of $\frac{1}{305,73}$; $\frac{1}{306,7}$; $\frac{1}{315,03} = \frac{1}{309,15}$; or the mean compression deduced from the mean degrees given by these three sections, compared with the French measure.

If we proceed in the same manner with the English and Swedish measures, we shall have by the whole as follows:

$$\text{By the French } \frac{1}{305,73}; \frac{1}{306,7}; \frac{1}{315,03}; \text{mean } \frac{1}{309,15}$$

$$\text{By the English } \frac{1}{310,28}; \frac{1}{311,36}; \frac{1}{318,97}; \text{mean } \frac{1}{313,54}$$

$$\text{By the Swedish } \frac{1}{305,14}; \frac{1}{305,72}; \frac{1}{310,72}; \text{mean } \frac{1}{307,19}$$

And the mean of the three means equal $\frac{1}{309,96} = \frac{1}{310,00}$ nearly for the compression at the poles, as deduced from these comparisons; which compression will be adopted for computing the different degrees from the equator to the pole.

All this is supposing the earth to be an ellipsoid. But that

these Indian measurements may rest on their own ground, I shall examine whether the increments to a succession of contiguous degrees as deduced from the present data, be consistent with the elliptic hypothesis, beginning with the degree in latitude $9^{\circ} 34' 44''$, as determined by observation. To effect this let $m^{(1)}$, $m^{(2)}$, $m^{(3)}$, &c. be the measures of *complete* contiguous degrees on the meridian in latitudes $l^{(1)}$, $l^{(2)}$, $l^{(3)}$, &c. Then, if a meridian of the earth be an ellipse, we know from equation 2, that the compression will be expressed

$$\text{by } \frac{m^{(2)} - m^{(1)}}{3 (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})} ; \text{ or } \frac{m^{(3)} - m^{(1)}}{3 (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})} ; \text{ or } \\ \frac{m^{(n)} - m^{(1)}}{3 (\text{Sin.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)})} ; \text{ let the length of the diameters be what} \\ \text{they will. So that we shall have } \frac{m^{(2)} - m^{(1)}}{3 m^{(1)} (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})} ; = \\ \frac{m^{(3)} - m^{(1)}}{3 m^{(1)} (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})} \cdot \text{ or } \frac{m^{(2)} - m^{(1)}}{3 m^{(1)} (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})} ; \\ = \frac{m^{(3)} - m^{(1)}}{3 m^{(1)} (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})} ; \text{ and by reduction } m^{(3)} - m^{(1)} = \\ = (m^{(2)} - m^{(1)}) \cdot \frac{\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}}.$$

$$\text{and } m^{(3)} = m^{(1)} + (m^{(2)} - m^{(1)}) \cdot \frac{\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} \\ m^{(4)} = m^{(1)} + (m^{(2)} - m^{(1)}) \cdot \frac{\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}} \\ m^{(5)} = m^{(1)} + (m^{(2)} - m^{(1)}) \cdot \frac{\text{Sin.}^2 l^{(5)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}}, \text{ \&c. to} \\ \text{to } m^{(n)} = m^{(1)} + (m^{(2)} - m^{(1)}) \cdot \frac{\text{Sin.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)}}{\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}}.$$

Also $m^{(2)} = m^{(1)} + (m^{(2)} - m^{(1)}) \cdot \frac{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}}$; that is $m^{(2)} = m^{(1)} + (m^{(2)} - m^{(1)})$, by preserving the expression $m^{(2)} - m^{(1)}$, which we will call d . Then we shall have $m^{(1)} = m^{(1)} + 0$

$$m^{(2)} = m^{(1)} + d$$

$$m^{(3)} = m^{(1)} + d \left\{ \frac{\sin. ^2 l^{(3)} - \sin. ^2 l^{(1)}}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}} \right\}$$

$$m^{(4)} = m^{(1)} + d \left\{ \frac{\sin. ^2 l^{(4)} - \sin. ^2 l^{(1)}}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}} \right\}, \text{ \&c.}$$

$$\text{to } m^{(n)} = m^{(1)} + d \left\{ \frac{\sin. ^2 l^{(n)} - \sin. ^2 l^{(1)}}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}} \right\}$$

Here d is the only unknown quantity to be determined, since $m^{(1)} + m^{(2)} + m^{(3)} \dots m^{(n)} = A$. the terrestrial measure of the arc of n complete degrees; $m^{(1)}$ being the measure of the first degree in latitude $l^{(1)}$ by observation.

$$\text{Then } A = nm^{(1)} + d \left\{ 0 + 1 + \frac{\sin. ^2 l^{(3)} - \sin. ^2 l^{(1)} \dots \sin. ^2 l^{(n)} - \sin. ^2 l^{(1)}}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}} \right\}$$

$$\text{And } d = \left\{ \frac{(A - nm^{(1)}) \cdot (\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)})}{(\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}) + (\sin. ^2 l^{(3)} - \sin. ^2 l^{(1)}) + \dots \sin. ^2 l^{(n)} - \sin. ^2 l^{(1)}} \right\}$$

when d becomes a known quantity. And since $\frac{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}}$ is a constant and known quantity, if $\frac{d}{\sin. ^2 l^{(2)} - \sin. ^2 l^{(1)}}$ be called

Q , we shall have the order of contiguous degrees as follows :

$$m^{(1)} = m + 0$$

$$m^{(2)} = m + d$$

$$m^{(3)} = m + Q \{ \sin. ^2 l^{(3)} - \sin. ^2 l^{(1)} \}$$

$$m^{(4)} = m + Q \{ \text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)} \} \&c.$$

$$\text{to } m^{(n)} = m + Q \{ \text{Sin.}^2 l^{(n)} - \text{Sin.}^2 l^{(1)} \}$$

To apply this formula to the present measurement, it will be necessary to have a terrestrial arc to correspond with the celestial one of complete degrees, and the first degree determined by observation. If we begin with the degree in latitude $9^\circ 34' 44''$, which is 60472,83 fathoms, as the mean degree deduced from an arc of $2^\circ 50' 10''$, 54, where the

corresponding terrestrial arc, is fathoms.
171516,75

The half of which is the distance of the middle

point of the arc from Patchipolliam, equal 85758,375

To which add half the degree south, or 30236,415

Their sum is the terrestrial arc between half the degree south of the middle point and

Patchipolliam, = 115994,790

The latitude of whose commencement is $9^\circ 4' 38'',66$

the latitude of the south extremity of an arc of complete degrees.

Now the terrestrial arc between Patchipolliam

and Namthabad is 248188,534

And between Namthabad and Daumergidda 178904,700

Their sum is the terrestrial arc between $9^\circ 34' 48''$

66 and Daumergidda 543088,024

The latitude of Daumergidda, by adding the arc between Namthabad and Daumergidda

($= 2^\circ 57' 23'',32$) by 13 stars, to the latitude of

Namthabad ($15^\circ 6' 0'',21$) gives 18°03'23'',64

The latitude of Daumergidda, by adding the whole arc between Punnæ and Daumergidda

($9^{\circ} 53' 45'', 25$) as determined by seven corres-

ponding stars, to the latitude of Punnaë, is $18^{\circ} 03' 23'', 64$

The mean of which, or correct latitude, is $18^{\circ} 03' 23'', 58$

Hence from $18^{\circ} 3' 23'', 58$

Subtract $9^{\circ} 4' 43'', 66$

Dif. or arc $8^{\circ} 58' 39'', 92$ whose measure is $543088,024$

To which add $0^{\circ} 1' 20'', 08$ whose measure is $1345,184$

Gives the $N^{\circ}. n$ of } $= 9^{\circ} 0' 0''$ whose measure (A) is $544433,21$
complete degrees }

Now the measure of the first degree is $60472,83$

And $n = 9$, therefore $nm^{(1)}$ is equal $544255,47$

Which subtracted from A gives $A - nm^{(1)} = 117,74$

And $\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)} = ,006014$; therefore, $,006014 \times 117,74$

$= 1,0689284 = (A - nm^{(1)}) \cdot (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})$ the

numerator; and the denominator $(\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) +$

$+ (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)}) + \dots$

$(\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})$ is equal $0,263137$. Hence we shall have

$$\frac{(A - nm^{(1)}) \cdot (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})}{(\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + \dots + (\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})}, \text{ equal}$$

$\frac{1,0689284}{0,263137} = 4,06225 = d$; and

$$= \frac{4,06225}{,006014} = 675,47 = Q$$
, and from these the following table

has been constructed.

TABLE I.

	Degrees.	Latitudes.
$m^{(1)} = m^{(1)} + 0$	60472,83	9° 34' 44"
$m^{(2)} = m^{(1)} + d$	60476,89	0 34 44
$m^{(3)} = m^{(1)} + Q (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})$	60481,34	11 34 44
$m^{(4)} = m^{(1)} + Q (\text{Sin.}^2 l^{(4)} - \text{Sin.}^2 l^{(1)})$	60486,16	12 34 44
$m^{(5)} = m^{(1)} + Q (\text{Sin.}^2 l^{(5)} - \text{Sin.}^2 l^{(1)})$	60491,36	13 34 44
$m^{(6)} = m^{(1)} + Q (\text{Sin.}^2 l^{(6)} - \text{Sin.}^2 l^{(1)})$	60496,92	14 34 44
$m^{(7)} = m^{(1)} + Q (\text{Sin.}^2 l^{(7)} - \text{Sin.}^2 l^{(1)})$	60502,85	15 34 44
$m^{(8)} = m^{(1)} + Q (\text{Sin.}^2 l^{(8)} - \text{Sin.}^2 l^{(1)})$	60509,12	16 34 44
$m^{(9)} = m^{(1)} + Q (\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)})$	60515,74	17 34 44
Sum	- 544433,21	

According to this table, the degree in latitude $16^\circ 34' 44''$ is 60509,12 fathoms; and the mean degree for latitude $16^\circ 34' 42''$, as deduced from the arc between Namthabad and Daumergidda is 60512,78 fathoms, which exceeds the computed one for latitude $16^\circ 34' 44''$ (which may be considered the same) only 3,66 fathoms.

It may however be necessary to notice that any one of the expressions $\frac{m^{(2)} - m^{(1)}}{3 (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)})}$; $\frac{m^{(3)} - m^{(1)}}{3 (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)})}$, &c. will bring out a compression equal $\frac{1}{269}$ nearly, which differs considerably from the general mean. But a very small difference in the numerator will produce a great difference in the compression.

If we suppose $\frac{r}{310}$ to be the true compression, let it be determined what the value of $m^{(1)}$ ought to be to bring out that compression; and by that means to detect the errors of the observed degrees, in latitude $9^\circ 34' 44''$, and $16^\circ 34' 42''$, which last may be compared with $m^{(8)}$.

Put $A = 544433,21$; $a = \text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)} = ,006014$, radius being unity; $b = (\text{Sin.}^2 l^{(2)} - \text{Sin.}^2 l^{(1)}) + (\text{Sin.}^2 l^{(3)} - \text{Sin.}^2 l^{(1)}) + \dots (\text{Sin.}^2 l^{(9)} - \text{Sin.}^2 l^{(1)}) = ,263137$.

Then $d = (m^{(2)} - m^{(1)}) = \frac{(A - nm^{(1)}).a}{b}$; and $\frac{d}{3m^{(1)}.a} =$

$= \frac{A - nm^{(1)}}{3b.m^{(1)}} = \frac{1}{310}$; from which is deduced $m^{(1)} = \frac{310.A}{3b + 310.n} =$

$= 60475,47$ fathoms. Whence $d = \frac{(A - nm^{(1)}) . ,006014}{,263137} = 3,5192$.

And $Q = \frac{d}{,006014} = 585,17$. From these the following table has been computed, from which it appears that the first degree by measurement, is 2,6 fathoms in defect; and that the one in latitude $16^\circ 34' 42''$ is 5,89 fathoms in excess; either of which is too small to affect the elliptic hypothesis; the greatest being only $\frac{1}{3}$ of a second on the earth's surface.

TABLE II.

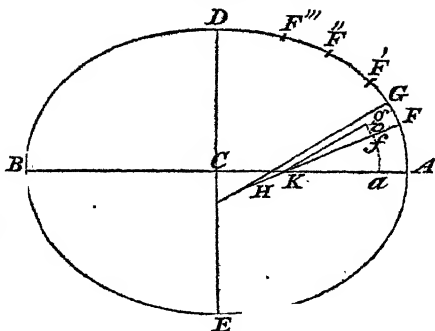
	Degrees.	Latitudes.
$m^{(1)} = m^{(1)} + 0$	60475,47	9 34 44
$m^{(2)} = m^{(1)} + d$	60478,99	10 34 44
$m^{(3)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(3)} - \text{Sin.}^2 \lambda^{(1)})$	60482,84	11 34 44
$m^{(4)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(4)} - \text{Sin.}^2 \lambda^{(1)})$	60487,02	12 34 44
$m^{(5)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(5)} - \text{Sin.}^2 \lambda^{(1)})$	60491,53	13 34 44
$m^{(6)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(6)} - \text{Sin.}^2 \lambda^{(1)})$	60496,34	14 34 44
$m^{(7)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(7)} - \text{Sin.}^2 \lambda^{(1)})$	60501,47	15 34 44
$m^{(8)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(8)} - \text{Sin.}^2 \lambda^{(1)})$	60506,91	16 34 44
$m^{(9)} = m^{(1)} + Q (\text{Sin.}^2 \lambda^{(9)} - \text{Sin.}^2 \lambda^{(1)})$	60512,64	17 34 44
Sum	544433,21	A

From inspecting these two tables, it appears that the degree in latitude $13^\circ 34' 44''$ is nearly the same in each, and the mean is 60491,46 fathoms; which certainly must be near the truth. I shall therefore adopt it with the compression $\frac{1}{510}$ for computing the general tables of degrees for every third degree of latitude from the equator to the pole.

With respect, however, to the compression, that nothing may be left undone to give full and entire satisfaction on this subject, I shall here add an investigation similar to that given by Professor PLAYFAIR, in the 5th vol. of the Edinburgh Philosophical Transactions, where, in place of using the measures of degrees due to particular latitudes, two measured arcs of large amplitudes are made use of, the latitudes of whose extremities have been determined with great accuracy.

Let ADBE be a meridian of the earth, where A is at the equator, and D at the pole.

Suppose F to be any point on that meridian, and FH the radius of curvature of the ellipse at that point. Put $AC = a$; $DC = b$; c being the centre of the ellipse; and let A be equal the angle AKF , the latitude



of F ; or let it be the measure of the arc of latitude aKf , to radius unity, or aK : that is, the measure of the angle aKf in parts of the radius aK , or unity. Let GF be an indefinitely small part of the ellipse. Then if $AF = z$, $GH = \dot{z}$ the fluxion of the arc AF of the ellipse; and if GH be drawn, then the angle $GHF = \angle gKf = \dot{A}$ or fg the fluxion of the arc of latitude aKf to radius 1. Hence as $1 : \dot{A} :: FH : \dot{z} = \dot{A}.FH$. But the radius of curvature $FH = a^2 b^2 (a^2 - a^2 \sin^2 A + b^2 \sin^2 A)^{-\frac{3}{2}}$. Let e be the ellipticity, or $a - b$; then $b = a - e$, and $b^2 = a^2 - 2ae$ very nearly, since e^2 is very small. Hence $FH = a^3 (a - 2e) \cdot (a^2 - 2ae \sin^2 A)^{-\frac{3}{2}}$. But $(a^2 - 2ae \sin^2 A)^{-\frac{3}{2}} = (a^2)^{-\frac{3}{2}} \cdot (1 - \frac{2e}{a} \sin^2 A)^{-\frac{3}{2}} = a^{-3} \cdot (1 + \frac{3e}{a} \sin^2 A)$ very nearly, by rejecting all the terms involving e^2 and its higher powers. Hence $FH = a^3 (a - 2e) \cdot a^{-3} \cdot (1 + \frac{3e}{a} \sin^2 A) = a - 2e + 3e \sin^2 A$, which substituted for FH , we get $\dot{z} = \dot{A}(a - 2e + 3e \sin^2 A) = \dot{A}(a - 2e) + \dot{A}(3e \sin^2 A)$. But $\sin^2 A = \frac{1 - \cos 2A}{2}$, and therefore $\dot{z} = \dot{A}(a - 2e) + \frac{3}{2} e \dot{A} - \frac{3}{2} e \dot{A} \cos 2A$; whose fluent is $z = (a - \frac{3}{2} e) A + \frac{3}{4} e \sin 2A = aA - e(\frac{A}{2} + \frac{3}{4} A \sin 2A)$ which requires no correction. And this is the measure of an

arc on the meridian extending from the equator to the latitude of the point F; where A denotes the arc of latitude in parts of the radius 1.

Let F' be any other point on the meridian, whose arc of latitude is A'. Then $AF' = a A' - e \left(\frac{A'}{2} + \frac{3}{4} A' \cdot \text{Sin. } 2A \right)$ and therefore $FF' = a(A' - A) - e \left\{ \frac{A' - A}{2} + \frac{3}{4} \cdot \text{Sin. } 2A' - \frac{3}{4} \cdot \text{Sin. } 2A \right\}$

Let F'', F''', be any other two points on the meridian whose respective arcs of latitude are A'' and A'''. Then from the same reasoning as above, we have $F''F''' = a(A''' - A'') - e \left\{ \frac{A''' - A''}{2} + \frac{3}{4} \cdot \text{Sin. } 2A''' - \frac{3}{4} \cdot \text{Sin. } 2A'' \right\}$

Now FF' and F''F''' are here supposed to be measured arcs on the meridian, whose respective lengths in fathoms may be called L and L', corresponding with the celestial arcs A' - A, and A''' - A''. To shorten the operation, put $A' - A = r$; $A''' - A'' = r'$. Also $\frac{A' - A}{2} + \frac{3}{4} \text{Sin. } A' - \frac{3}{4} \text{Sin. } A = S$, and $\frac{A''' - A''}{2} + \frac{3}{4} \text{Sin. } 2A''' - \frac{3}{4} \text{Sin. } 2A'' = S'$. Then we have $L = ar - es$; $L' = ar' - es'$. And therefore $a = \frac{s'L - sL'}{rs' - r's}$; $-e = \frac{r'L - rL'}{rs' - r's}$; and $\frac{e}{a} = \frac{r'L - rL'}{s'L - sL'}$ equal the compression expressed in fractional parts of the semi-equatorial diameter.

To apply this to the case in question,

Let A = the latitude of Punnae = $8^{\circ} 9' 38.4''$

A = the lat. of Daumergidda = $18^{\circ} 3' 23.6''$

A' - A = r = $9^{\circ} 53' 45.2'' = 1727158$

A'' = lat. of Montjouy = $41^{\circ} 21' 44.96''$

A''' = lat. of Dunkirk = $51^{\circ} 02' 09.2''$

A''' - A'' = r' = $9^{\circ} 40' 24.24'' = 1688227$

$$\text{Put } s = \frac{A' - A}{2} + \frac{3}{4} \text{Sin. } 2 A' - \frac{3}{4} \text{Sin. } 2 A = , 3176258$$

$$s' = \frac{A''' - A'}{2} + \frac{3}{4} \text{Sin. } 2 A''' - \frac{3}{4} \text{Sin. } 2 A' = , 0738689$$

$$\begin{array}{l} L = 598610 \\ *L' = 587475,41 \end{array} \left. \begin{array}{l} \text{arc be-} \\ \text{tween} \end{array} \right\} \begin{array}{l} \text{Punnæ and Daumergidda,} \\ \text{Montjouy and Dunkirk.} \end{array}$$

$$a = \frac{s'L - sL'}{rs' - r's} = 3483955$$

$$c = \frac{r'L - sL'}{rs' - r's} = 9820,8$$

$$\frac{c}{a} = \frac{r'L - sL'}{s'L - sL'} = \frac{1}{355} \text{ nearly.}$$

In the paper which I sent to the Asiatick Society, and which will appear in the 13th volume of their Researches, the terrestrial arc between Barcelona and Dunkirk, as given in the 2d volume of Colonel MUDGE's Survey, was made use of, and is there stated to be 587987 fathoms, which gives the compression by this method $\frac{1}{272}$. But there must be some mistake in this; for by comparing it with the distance between Montjouy and Dunkirk, as given by DE LAMBRE, the former is considerably greater than the latter, though Montjouy is 3" south of Barcelona. The mean degree for latitude $47^{\circ} 24'$ used in that paper for determining the ellipticity, compared with the Indian measurements, was deduced from that arc, and gave the compression $\frac{1}{291,6}$, while the general mean compression obtained by comparing these measurements with the French, English, and Swedish degrees, was $\frac{1}{304}$ nearly.

Since it is here determined to adopt $\frac{1}{310}$ as the compression, and 60491,46 fathoms for the measure of the degree

* See vol. iii. p. 89. Base du Systeme Métrique, where the arc between Montjouy and Dunkirk is 551583,6 toises, or 587657,17 fathoms, at the temperature of 54° , which reduced to the temperature of 62° , will be 587475,41 fathoms.

on the meridian, due to latitude $13^{\circ} 31' 41''$; we shall have $m' = 60491.46$; $l = 13^{\circ} 31' 41''$. Then if m be the degree in any other latitude l ; by equation 5, $m = \frac{1 + 3e \cdot \sin. l}{1 + 3e \cdot \sin. l} \cdot m'$. If m have its middle point on the equator, where $l = 0$, then

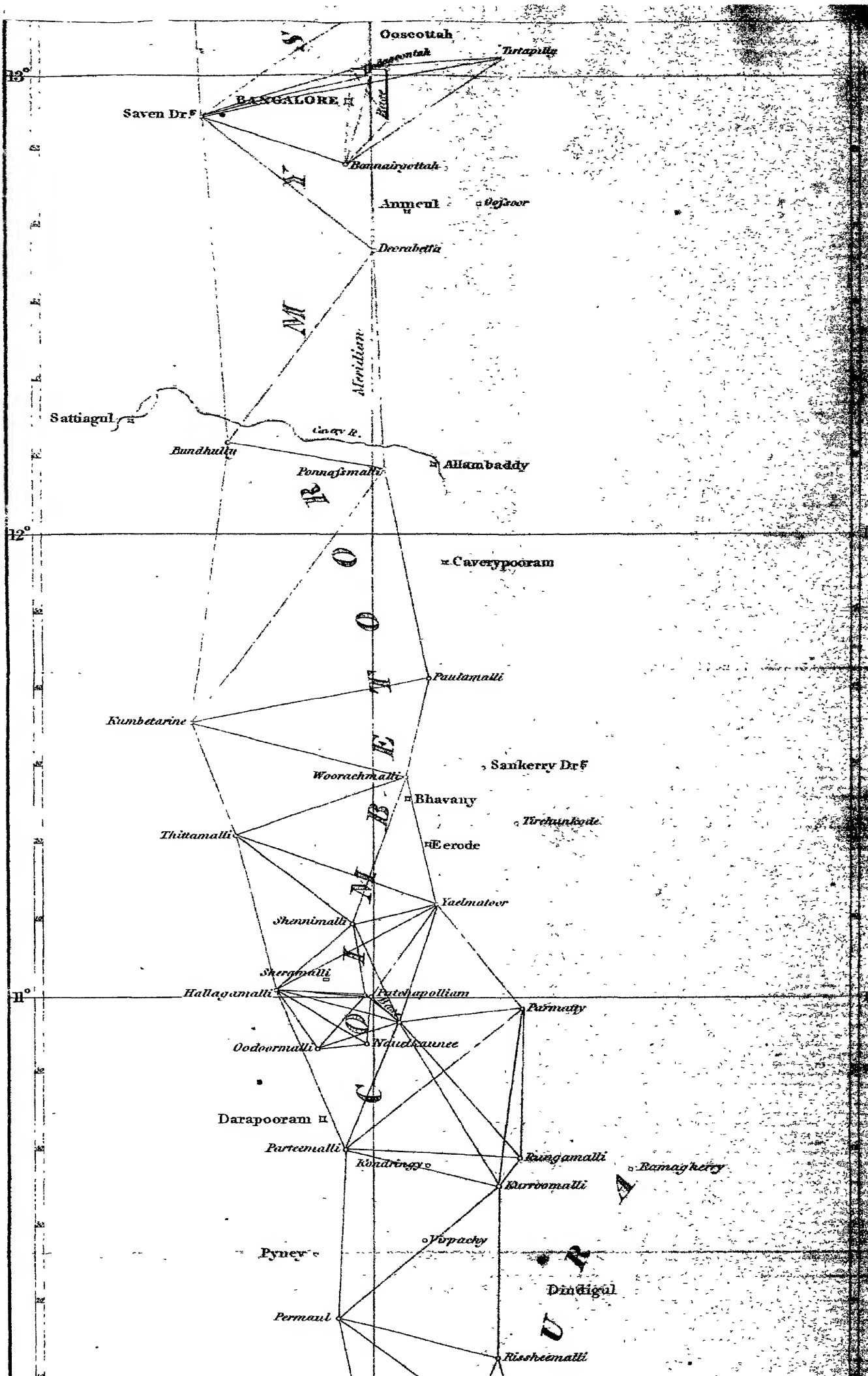
$$m = \frac{m'}{1 + 3e \cdot \sin. l} = \frac{60491.46}{1.00053345} = 60459.2 \text{ fathoms.}$$

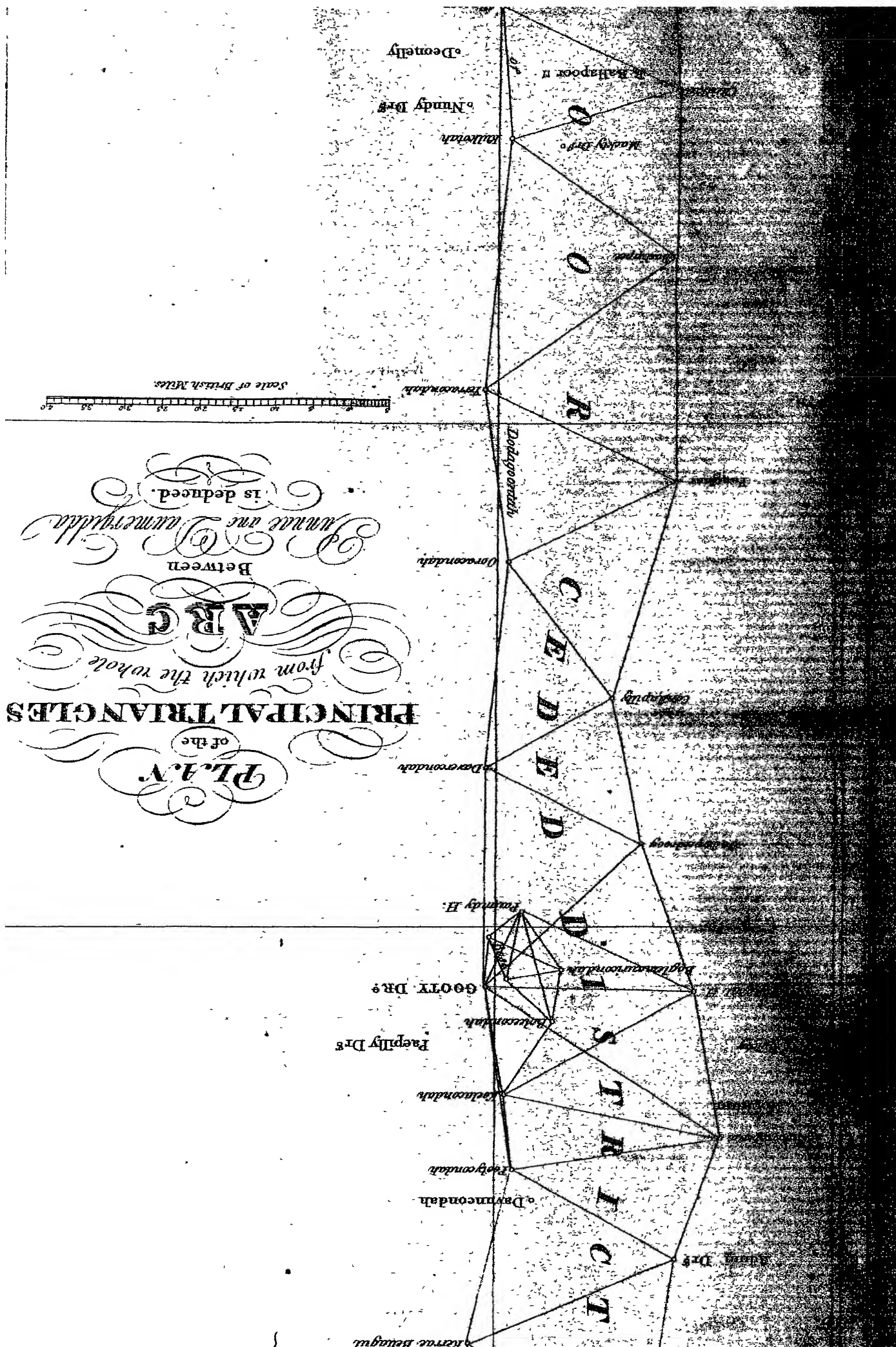
By equation 16, $p = m \cdot \frac{1 + 3e}{1 + e} = 60459.2 \cdot \frac{1 + 3e}{1 + e} = 60459.2 \times 1.006431 = 60848$ fathoms for the degree on the equatorial circle. Put $A = 57,2957795$ the arc equal radius. Then $A \cdot p = 57^{\circ}, 8\&c. \times 60848 = 3486331 = \frac{1}{2}a$; and therefore $a = 6972668$ fathoms; and consequently $b (= a \cdot (1 - e)) = 6972668 \times 9967742 = 6950176$ fathoms, the length of the polar axis. Now since 6972668 is the diameter of the equatorial circle, then 3,14159, &c. multiplied by 6972668, gives 21905280 fathoms for the circumference of the circumscribing the elliptic meridian. Put $d = 1 - \frac{b^2}{a^2} = .00644$. Then as $1 : 1 - (\frac{d}{2} + \frac{3d^2}{2 \cdot 4} \&c.) :: 21905280$, the circumference of the circumscribing circle : 21869976 = the circumference of the elliptic meridian; which, divided by 4, gives 5467494 fathoms for the quadrantal arc of that meridian; and this reduced into inches, and divided by 10,000000, will give 39.366 inches for the French mètre, at the temperature of 62° . Now, the mètre deduced from the measurements of DE LAMBRE and MECHAIN, and reduced from 32° to 62° , was 39,371, English inches, which exceeds this one by .005 inches: a quantity too small to affect any standard measure: so that the mètre as deduced from a comparison of all the recent operations, may be considered, as to practical purposes, the same as that which has been

adopted by the French mathematicians, being obtained from comparing the measurements of DE LAMBRE and MECHAIN, with those of BOUGUER and CONDAMINE.

As I am in hopes that another section, and perhaps more, will be added to the arc, I shall defer making any final conclusions till I see what may be done. The next station of observation, I propose to be as near the latitude of $21^{\circ} 6' 5''$ as possible, in order that the middle point of the section may fall in $19^{\circ} 34' 44''$, so as to compare the mean degree as obtained by observation, with the one computed from the increments as in the foregoing tables. I say another section, and *perhaps more*; because should the country to the northward be open and settled, there may be a possibility at some future day, of continuing the same arc to the northern confines of Hindostan: so much, at least, seems necessary for laying the foundation of Indian geography; and if it were conducted with zeal and judgment, it would not be a work of many years, provided the features of the country be favourable. The whole time taken up in the measurement of the arc between Punnæ and Daumergidda, including the ~~base~~ lines, astronomical observations, &c.; that is to say, the entire field work, has only been three years and nine months; and a considerable part of the corrections for the stars, for the angles, and for the reduction of the base, were done during the time of measuring the base and observing for the zenith distances; so that I suppose *four years and a half* may be allowed for the whole work. From this estimate, the meridional arc might be continued from Daumergidda to Dhelli in about five years, if no local impediment disturbed its progress. It is however probable that difficulties might occur in Sindia's country,

which a northern direction from Daumergiddi would render it necessary to pass through. But it would be a sufficient point gained, if a series of triangles were carried from Nagpoor in Berar, to Kalpy on the Jumna, which two places, if the maps are correct, lie nearly on the same meridian. From Kalpy, the meridional series might perhaps be continued north to the Kemaon mountains; Kalpy would also be a favourable position from which to extend a series to the east and west, and for meridian stations not more than sixty or seventy miles from each other, where the positions of the meridians ought to be determined by pole star observations. Data would then be had for extending the survey on a more enlarged scale over the upper provinces; and the arc between Nagpoor and Kalpy might be easily reduced to the one terminating in latitude $21^{\circ} 6'$ (that station of observation and the one near Nagpoor being connected), so as to form one entire arc between the parallels of Punnae and Kalpy. Thus would be formed a geometrical connection between the southern and northern possessions of the East India company, and a complete basis laid for local and detailed surveys of the whole. Innumerable might be the individuals employed in carrying them into effect, and various might be the description of these surveys. By the assistance of this work as a foundation, they might all be rendered useful; but without it, no combination of the best common surveys could ever be formed into a correct map to embrace such an extensive territory as





The lengths of different degrees computed from the foregoing data, for every three degrees from the equator to the pole.

Lat.	Degrees on the meridian.	Degrees on the perpendicular.	Degrees of Longitude.
0	60459,2	60848,0	60848,0
3	60460,8	60848,4	60765,0
6	60465,6	60850,1	60516,8
9	60473,5	60852,8	60103,6
12	60484,5	60856,5	59526,7
15	60498,4	6086 ,1	58787,3
18	60515,1	60866,7	57887,7
21	60534,3	60873,2	56830,0
24	60556,0	60880,5	55628,1
27	60579,8	60888,5	54252,0
30	60605,5	60897,1	52738,4
33	60632,7	60906,2	51080,2
36	60661,3	60915,8	49281,9
39	60690,8	60925,7	47348,2
42	60721,3	60935,7	45284,0
45	60751,8	60946,1	43095,4
48	60782,3	60956,4	40787,8
51	60812,5	60966,5	38367,5
54	60842,1	60976,5	35841,1
57	60870,7	60986,1	33215,4
60	60898,0	60995,2	30497,6
63	60923,7	61003,8	27695,2
66	60947,5	61011,8	24815,7
69	60969,1	61018,9	21867,2
72	60988,3	61025,6	18857,9
75	61005,1	61031,6	15796,0
78	61018,9	61035,8	12690,1
81	61029,9	61039,5	9548,7
84	61037,8	61042,1	6380,6
87	61042,6	61043,7	3194,8
90	61044,3	61044,3	—

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| Urh-Cheh-Tsze-Teën-Se-Yin-Pe-Keaon, being a parallel drawn between the two intended Chinese Dictionaries, by the Revd. Robert Morrison, and Antonio Montucci, LL.D. London, 1817. 4° | Dr. Montucci. |
| Commentationes Societatis Regiæ Scientiarum Gottingensis Recentiores. Vol. III. ad A. MDCCCXIV-XV. Gottingæ, 1816. 4° | The Royal Society of Sciences of Gottingen. |
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